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by
J. W. N. SULLIVAN

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PREFACE

As a result of reading, for some years past, popular expositions of relativity theory, I have become convinced that most of these books fail in their purpose because they use an inappropriate technique. The technique of popular exposition of a scientific theory became standardised, as it were, in the nineteenth century. What may be called the "pictorial method" was definitely adopted. Every fact that had to be communicated was, as far as possible, presented in terms that appealed to the imagination. The method is still successful in many expositions. Thus an atom is presented as a "miniature solar system" with the result that ordinary readers have no difficulty in understanding atomic theory — provided that the quantum theory is not mentioned. A way of presenting quantum theory pictorially has not yet been invented. Now Einstein's theory is peculiarly recalcitrant to this kind of exposition.

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It is quite justly described as a highly abstract theory. Its purpose is to present a new way of co-ordinating the phenomena studied in the science of physics. Its actual content is to be found in the fundamental entities it assumes and in the relations it establishes between these entities. These entities and these relations are different from those hitherto used in physics. Their advantages over the old ones are that they are fewer in number, and that their adoption leads to a greater unification and comprehensiveness on the part of the science of physics. An exposition of relativity, therefore, should be chiefly concerned to show what the new entities are and how they differ from the old ones. An ideal exposition would also show how much greater a unity is bestowed on physics by their adoption, but I do not think this is possible in a non-technical exposition. Accordingly I have endeavoured, in the following exposition, to describe the new entities introduced by Einstein and their relation to the old ones introduced by Galileo and Newton. Unfortunately these entities are abstractions — like Newton's. But while we are familiar with

Newton's abstractions, force, mass, and so on, we are not familiar with such abstractions as a non-Euclidean four-dimensional continuum. Also Einstein's abstractions were introduced for the same reason that Newton's were introduced, namely, that they are susceptible of mathematical treatment. For physics tries to describe phenomena only in terms of entities that can be mathematically defined. Hence, in discussing Einstein's entities, I have done so with reference to their mathematical aspects. The reader must be resigned to the fact that it is only in virtue of their mathematical aspects that these entities have any *raison d'être* at all. Nevertheless, although I am forced to talk about mathematical entities, I do not perform any mathematical calculations. It would be necessary to do so if I wanted to show how the theory of relativity develops from its assumptions. But my object is not to present the theory of relativity, but to show what the theory is about.

In the Introduction I make clear one or two phrases that constantly recur in the rest of the book, and which it would need a great deal of

circumlocution to avoid. The mathematical appendix gives the mathematical expressions of the chief results of the theory, and a sketch of the reasoning by which they were obtained. It is designed for those readers, who seem to me fairly numerous, who studied mathematics in their youth, but whose present mathematics is not, as it were, sufficiently bright and ready to enable them to gain their knowledge of relativity from the original sources. Here again it is chiefly the general hang of the matter that I have tried to communicate. My own indebtedness is chiefly to Prof. Eddington's *Mathematical Theory of Relativity*. It is easily the best exposition I have read, and I have adhered to it almost slavishly in the following pages.

J. W. N. Sullivan.

Chobham, 1924.

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INTRODUCTION

§ 1

The Scientific Description of the Universe

SCIENCE is an attempt to describe a certain region of experience in terms of certain fundamental entities and principles. It is concerned with a limited region — not, perhaps, as a necessary consequence of its own nature, but certainly as a matter of historical fact. Certainly the *exact* sciences, if we consider the whole of any human being's experience, have a very limited range. The behaviour of sticks and stones, stars and planets, are fairly well understood — in certain aspects, at least. But when we come to the phenomena of life and, still more, of mind, it becomes apparent that science, so far as it deals with such things at all, tells us nothing in comparison with what there is to be known. It is not too much to say that all that is most im-

portant to man lies at present outside science. The kind of information science gives us about the "material universe," however, is quite worth having. Explanations of material happenings in terms of "force," "mass," and so on, really do meet the kind of interest we take in these phenomena. In terms of these primary concepts science can describe these phenomena in an orderly and coherent way which is also satisfactory. Descriptions of the phenomena of life in terms of these same concepts are not so satisfactory. Some biologists find that they have to introduce new primary concepts which cannot be reduced to those found adequate for the purposes of physics and astronomy. Hence we find such terms as "vital," "purpose," "individuality" in the writings of biologists. There are some scientific men who think that all these terms will prove translatable into the terms used in physics—that the terms found adequate for mechanical systems will ultimately prove adequate for the phenomena of life, and even for mental phenomena. It must be remembered, however, that science not only progresses; its fundamental prin-

ciples and entities alter. It is extremely likely that the sciences of life and mind of the future will adopt very different entities and principles from those used in physics at the present day.

In the history of science various principles and entities have been adopted to describe even the "material" universe. The ancients thought that the planets moved in circles because the circle was a perfect figure. Heavenly bodies, of course, could only move in perfect figures. This is an æsthetic principle, and is not now used in astronomy. On the other hand the ancient astronomers did not adopt an entity, namely, *inertia,* which modern astronomers find very useful. Their whole description of astronomical phenomena was based on principles and fundamental concepts which we have replaced by a different set. Again, in the physics of the early Renaissance, water was supposed to rise in a pump because nature abhorred a vacuum. We now explain the same phenomenon by referring it to the pressure of the atmosphere. Even as late as Descartes, celebrated mathematicians deduced the laws of mechanics from the perfections

of God. We now know less about the nature of God and prefer to deduce mechanical principles from observation. These examples, which could be multiplied, are sufficient to show that the physical sciences have not always adopted the same principles nor assumed the same fundamental concepts in the attempt to describe nature. It cannot be too clearly realised that a science is a system of making orderly and coherent a certain region of experience, and that the terms in which it does so are to some extent arbitrary. The general beliefs current in any particular age are largely responsible for the principles and entities adopted by men of science.

The concepts of modern physics are largely due to Newton. As specimens of these concepts we may instance mass, inertia, force. These concepts, as Newton defined them, were the crystallisation of a great deal of earlier thinking. They are all abstractions, attempts to arrive at a few fundamental terms in which certain observed phenomena could be conveniently and economically described. In terms of these concepts the motions of the planets, for example, could be described

in an orderly fashion. An earlier attempt to describe the planetary motions stated that angels carried them round the earth in just the way they were observed to go round. But this theory is less satisfactory than Newton's because the planetary angels play no part in other phenomena. They do not explain, for instance, the phenomena of falling stones or the behaviour of two colliding bodies, whereas Newton's entities suffice to describe a very large number of different phenomena. Since Newton's time some of his entities have acquired a richer content, some have been abandoned, and some new ones have been introduced. The notion of "energy," for example, has changed a great deal since Newton's time. The absolute time and absolute space in which he believed are not now used in physics. Electricity is an example of a new concept. Science, therefore, not only tries to bring a greater and greater region of experience within its scheme, but it also gradually modifies its scheme. Even its fundamental principles undergo modification. Thus the principle of continuity has been replaced, in large regions of physics, by the principle of discontinuity. But

it would be a mistake to suppose that changes in the scientific scheme are purely capricious. We can trace a direction in these changes and we can say that they have gradually converged towards a system of interpretation where none but *observable* factors are considered as in causal dependence. We must not interpret the word "observable" too narrowly in this statement. It would be more correct to substitute for "observable," "definable in terms of physical processes." If an entity is to be considered as a scientific entity we must be able to say what physical processes would enable us to detect it. This is the basis of Einstein's objection to Newton's absolute space and absolute time. There are no physical operations, according to Einstein, which enable us to distinguish absolute space. As regards absolute time, Newton himself confessed that there may be no natural processes which enable us to measure it. We can never, in the nature of things, say whether we are dealing with absolute time or not. Both these entities, therefore, are described by Einstein as metaphysical, with no real place in science. Newton said that

the centrifugal force developed by a rotating body was due to the body's relation to absolute space. Here an unobservable factor, absolute space, is invoked as the cause of an observable physical phenomenon. According to Einstein, science must not invoke such an entity as the cause of anything. Absolute space and time, so far as science is concerned, belong to the same class of entities as the Will of God, Beauty, the Principle of Evil, and so on. They may even be realities, and some kinds of knowledge may find it necessary to assume them, but since they are not definable in terms of physical processes, since we know of no physical apparatus which can measure them or even detect their existence, they are not to be imported into scientific descriptions. In other words, science is a game played according to certain rules. It is quite open to anybody to say, as doubtless some mystics would, that such rules exclude everything of real interest about the universe. Science, however, is committed to them, and the choice must be justified by its results.

§ 2

Co-ordinates

Everything with which science is concerned takes place in space and time. Of every event that any man of science ever examined we can say that it took place somewhere and somewhen. It is perfectly possible that there are some things for which the conceptions of spatial and temporal location are inadequate. In science, however, nothing is considered but what exists in space and time. It must be noted that we refer to existence in *both* space and time. We do not consider anything that exists at no time at all, nor do we consider anything that exists nowhere. Consequently, in describing an event, we have to say both when and where it happened. It is essential, therefore, that we should have a method whereby we can assign unambiguously to every event both its place and its time. Methods of assigning the place and time of anything vary. We may assign the position of an object by saying that it is opposite to the Law Courts, and the

time of an occurrence by saying it happened in the year that Captain Cuttle won the Derby. We should not employ this method, however, if we were talking to a foreigner. We want less local standards from which to make our measurements. We could, for instance, give the latitude and longitude of a place, and the date of an event. In any case we must have an arbitrary starting point, such as the meridian at Greenwich or the beginning of the Christian era. The most popular method in science of specifying the place of an event is the method of Descartes. Consider, for instance, the position of a point on a plane piece of paper. We first draw two straight lines at right angles to one another. The position of the point can be exactly specified by giving its perpendicular distance from each line. These perpendicular distances are called its *co-ordinates,* and the two fixed lines at right angles are called *axes of co-ordinates.* In this case the two fixed lines are said to form a rectangular system of co-ordinates. We can extend this method to points not lying in the plane of the paper. We might, for instance, have a stick planted on a

tennis lawn and wish to determine the position of the knob. Taking the base line and a side line as our axes of co-ordinates we can give the two distances of the point where the stick is planted in the ground from these two lines. In addition, to determine the position " in space " of the knob, we must give the height of the stick. For the position of a point in space, therefore, we require three measurements. This is what is meant by saying that space has three dimensions. The reader will find that, whatever method he invents, he will not be able to determine the position of a point in space with less than three measurements. We usually select three planes at right-angles, and give the perpendicular distances of the required point from each of these planes. We cannot, of course, have more than three planes or three lines at right-angles. We can put three matches so that each is at right angles to the other two, but we cannot possibly put a fourth at right angles to the other three.

We need not always use rectangular axes. We might, for instance, in a plane, take a fixed line and a fixed point on it. Then the position of any

other point in the plane could be determined by drawing a line from that point to the fixed point. The length of the line and the angle it makes with the fixed line are then the two measurements required. In space we could use an analogous method. We should require one distance and two angles to determine the position of a point. Such co-ordinates are called polar co-ordinates.

Co-ordinates of some kind or another are continually being used in the mathematical sciences. In astronomy, for example, it is very usual to imagine three enormous planes meeting in the centre of the sun. The actual directions of these planes are, of course, properly defined. The position of any body in the solar system, a planet, a comet, or what not, is then expressed in terms of its perpendicular distances from these three planes.

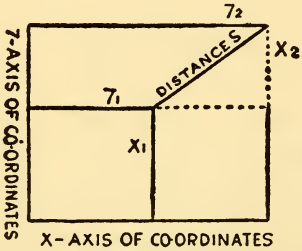
We have said that, to specify an event completely, we require to know the when as well as the where of its occurrence. Measured from some "zero" time, therefore, we have to give the time as well as the place. In general, as we have seen,

it requires three measurements to determine the place of an event. We require one more measurement — a time measurement — to determine the time of its occurrence. In general, therefore, we require four measurements altogether. By a slight extension of the meaning of the term we can call the time measurement also a "co-ordinate." Fully to specify an event, therefore, we require four co-ordinates. We may mention that space co-ordinates are generally denoted by x, y, z, and the time co-ordinate by t. Thus, if we are using rectangular axes, and our units are feet and seconds, we might say that the co-ordinates of an event were $x = 2$, $y = 3$, $z = 4$, and $t = 7$. This means that the place of the event was 2 feet distant from one plane, 3 feet from another, and 4 feet from the third, and that the event occurred 7 seconds after the moment chosen as zero time.

There is one very easy but very important problem that arises in connection with the use of co-ordinates. That problem is to determine the distance between two points whose co-ordinates are given. Consider, for example, two points in

a plane. Let their co-ordinates be x_1, y_1 and x_2, y_2. What is the distance of the point x_2, y_2 from the point x_1, y_1?

Consider the triangle in the diagram formed by the distance s and the two dotted lines. The horizontal dotted line is obviously, in length, $x_2 - x_1$. The vertical dotted line is, in length, $y_2 - y_1$. Now we know from Euclid that the square on the hypotenuse of this triangle is



equal to the sum of the squares on the two sides. The hypotenuse is the distance s between the two points. Hence $s^2 = (x_2 - x_1)^2 + (y_2 - y_1)^2$ or $s = \sqrt{(x_2 - x_1)^2 + (y_2 - y_1)^2}$. This is the simple formula for the distance between two points in a plane whose co-ordinates are given. An analogous expression gives the distance between two points in space, *i.e.*, two points each of which has

three co-ordinates. Suppose these co-ordinates to be x_1, y_1, z_1 and x_2, y_2, z_2 respectively. Then the distance s between the two points is given by the formula $s^2 = (x_2 - x_1)^2 + (y_2 - y_1)^2 + (z_2 - z_1)^2$.

It must be remembered that these formulæ only hold good for rectangular systems of co-ordinates. Different formulæ are required for other systems.

§ 3

Infinitesimals

It is often convenient, in mathematical calculations, to consider the distance between "infinitely near" points. The phrase "infinitely near" is a little unfortunate, since it suggests a number of logical puzzles to the reader. It is only a way of speaking, however. We may illustrate the sort of idea involved by thinking of the motion of a falling body. The body falls more and more quickly, so that its velocity is constantly changing. Nevertheless, one is quite justified in saying that the body has a definite velocity at every instant of its fall. How can we find its velocity

at any given instant? The usual way of finding a velocity is to divide the distance traversed by the time taken to do it. But in the present case, whatever distance we take, the velocity has changed during that distance. The method would give us only an average velocity. Nevertheless, the smaller the distance we take the nearer we shall get to the velocity we want. We could imagine ourselves taking smaller and smaller distances, dividing them by the smaller and smaller times involved, and so getting nearer and nearer to the required velocity. Is there any limit to this process? The only limit possible would seem to be when the distance and the time taken to describe it were both zero. Nevertheless, there is a method, called the differential calculus, for finding the ratio of two quantities as they both decrease without limit. We shall not describe this method. We only mention it to make clear the notation to be employed. Thus, if we call a distance s and the time taken to describe it t, then we obtain a velocity from the ratio $\frac{s}{t}$. But when mathematicians wish to tell us that they

are considering a limiting velocity, such as we have described above, they write the ratio $\frac{ds}{dt}$. The "d" used here is not a quantity multiplying s and t. It is merely a short-hand way of indicating that s and t are being considered at the moment that they vanish. It is rather illogical, but it is also convenient, to picture ds as an exceedingly small distance and dt as an exceedingly small time. In this pictorial sense ds and dt are called infinitesimals. The points separated by the distance ds are said to be infinitely near together. It is all a mere manner of speaking, just to give our imaginations something to go upon. When a mathematician talks about infinitely near points or infinitesimal distances and times, we need have no logical scruples in listening to him. All that he really means to say is that he is employing the method of the differential calculus, but that he likes to try to *picture* what he is doing as he goes along.

The geometrical properties of a complicated surface may vary from point to point. In this case it is convenient to have an expression for

the distance between two infinitely near points. And in order to adopt a uniform method we may, in all cases, express the formula for distance in this infinitesimal notation. Thus in rectangular axes the distance between the x co-ordinates of two infinitely near points in a plane may be written dx. Similarly, for the distance between the y co-ordinates we may write dy. And the distance between the two infinitely near points is given by the formula $ds^2 = dx^2 + dy^2$. The analogous expression for three dimensions is $ds^2 = dx^2 + dy^2 + dz^2$.

§ 4

Equations

We have said that the science of physics adopts entities which are more or less arbitrary in order to give a coherent picture of a certain region of experience. The great guiding principle in the selection of these entities is that they shall be susceptible of mathematical treatment. Such notions as force, velocity, acceleration, and so on, can all be defined mathematically. The purpose of physics is to describe phenomena by means

of equations and, during the last hundred years, physics has come to describe phenomena more and more in terms of what are called "differential equations." Suppose, for instance, that we have a region of space permeated by the waves of wireless telegraphy. These waves are said to be electro-magnetic waves, which means that at every point of the space considered there is an electric force and a magnetic force. What is the relation between these forces? We must remember that they vary from point to point of the space considered, and they also vary from moment to moment of time. Let us confine our attention to a certain point at a certain moment. We can find, by the method of the differential calculus, the change in the forces as we pass from the point considered to an infinitely near point. We can also find the change they undergo in an infinitesimal lapse of time. We try to find the relations between these changes. We may find, for instance, that the space-rate of change of the magnetic force is equal to the time-rate of change of the electric force. And we may see that this result is general, that it holds good at every point

of the space considered, and for every moment of time. We are then said to have found an equation and, moreover, since it is infinitesimal stretches of space and infinitesimal lapses of time with which we are concerned, our equation is a differential equation. From these equations we can, by mathematical development, find all the laws these waves must obey.

These equations, since they express rates of change with respect to distance and time, assume, of course, some system of co-ordinates. But there is an infinite number of varieties of systems of co-ordinates. Must we have different equations, then, for every system of co-ordinates that anybody might choose to employ? Einstein answered this question by saying that the equations of physics are only correctly formulated when they remain unaltered for any change in the co-ordinate system. This is a very strict criterion. It rules out, for instance, Newton's equations to motion, although it leaves standing Maxwell's electro-magnetic equations. Equations which satisfy this criterion are called Tensor equations, and they are treated by the Calculus of Tensors.

FIRST DIALOGUE

THE NEWTONIAN PHILOSOPHY

(*The dialogue takes place between a* MATHEMATICAL PHYSICIST, *a* PHILOSOPHER, *and an* ORDINARY INTELLIGENT PERSON. *The* MATHEMATICAL PHYSICIST *is provided with a black-board, on which he occasionally writes down very simple mathematical expressions. The* ORDINARY INTELLIGENT PERSON'S *first impulse, on seeing these expressions, is to take fright and to give up all attempts to follow the argument. Encouraged by the benign placidity of the* MATHEMATICAL PHYSICIST, *however, he takes a second look at the expressions, and finds, to his great surprise and relief, that he can understand them perfectly well.*)

Phil. (*to Math. Phys.*): I think it would be advisable, in expounding to us the new theory of the universe which is associated with the name

of Einstein, that you should begin by making clear to us the theory that it has replaced, the theory that is associated with the name of Newton. Einstein's theory is, I understand, an alternative way of accounting for phenomena, and its strength lies in the greater simplicity and naturalness of its assumptions. If, therefore, you first make clear to us the assumptions involved in the older theory we shall be in a better position to appreciate the simplicity which, you say, Einstein's theory introduces.

Math. Phys.: Exactly. And that is what I propose to do on this occasion. The assumptions of the Newtonian philosophy have dominated physics up to within quite recent times. Indeed, it was not until 1905, when Einstein published his restricted principle of relativity, that most of us realised that they were assumptions at all. We regarded them, if we thought about them at all, as necessities of thought. It is true that, even in Newton's time, objections were made to some of his assumptions. Leibniz for instance ——

Ord. Int. Per.: What are these assumptions?

Math. Phys.: They are numerous. Newton assumed, for instance, three fundamental entities, Time, Space, Matter. Time he conceived as existing independently of both space and matter. Time, he appeared to think, could go on even if the whole universe shrank to a point. Space, however, required time. A space which lasted for no time at all was inconceivable. Matter required both time and space. Matter must exist somewhen and somewhere. But these three entities were regarded as independent in the sense that matter merely " occupied " time and space. Space remained exactly the same whether it was occupied by matter or not; none of its properties were altered by the presence of matter. And the same was true of time. Time was not merely a relation between events. Events occupied time, and by so doing acquired the relation of before and after. But time would go on even if there were no events. Similarly, the distance between two bodies was a relation they acquired by existing in space. The bodies occupied different parts of an independently existing space, and the distance between the immovable parts of space they occupied at a

given instant — an instant of the independent and uniformly flowing time — was the distance between the bodies at that instant.

Phil.: Well, I see no objection to this description of Time and Space. Of course, Newton doubtless supposed that his Time and Space were objective entities, and that they were facts of nature quite independent of the mind. In this he was wrong. Space and Time are contributed by the mind itself; they are forms of perception. But, granted that, I do not see why they should not have the properties that Newton describes.

Math. Phys.: You will see, if you have patience and listen to me long enough, that I also agree that Space and Time are contributed by the mind — matter too, in point of fact — but my present objection to the Newtonian Space and Time is not that. My objection is this: How can one distinguish this Space and Time? Consider, for instance, the distance between two material point particles. Their distance, according to Newton, is the distance between the points of space at which they are situated. In measuring the distance we measure the distance between the two

bodies. Why, then, should we not simply say that their distance is a relation between the two bodies? Why introduce these hypothetical immovable points of space *at which* the bodies are situated?

Ord. Int. Per.: I think I can answer that. Bodies move about. There is no such thing as *the* distance between two bodies. Suppose I want to state the distance from a body at one moment to another body at another moment. I must state the distance between the point of space the one body was at at one moment, and the point of space the other body was at at the other moment.

Math. Phys.: But how do you identify these points of space? In practice you use a frame of reference, constituted by material bodies. You find that your first body coincides with a certain mark on your scale at an instant t_1, and the second body with another mark at an instant t_2. And in doing this you make certain assumptions about your mode of measuring time. The whole question is a little complicated, and I must deal with it later.

Phil.: It is complicated, you say. Well, the

Newtonian theory has the merit of dealing simply, at any rate, with the case our young friend has just stated.

Math. Phys.: The fact that the Newtonian theory is simple does not help us if it cannot be applied. I do not know what concepts are considered admissible in philosophy, but in science we are endeavouring to use no concepts which cannot be defined in terms of physical processes. The distance between two bodies, for instance, may be defined in terms of physical operations of measurement, but the distance between two points in space cannot be so defined. You have to introduce bodies which are *at* the points. This must mean that you know whether or not a body is in a state of absolute rest. I admit that the Newtonian philosophy hangs together. If you have absolute space and absolute time you can have absolute motion. But can you tell me of any physical experiments which enable you to know whether you are dealing with a case of absolute motion?

Phil.: Surely the laws of motion have been stated by Newton.

Math. Phys.: Yes. And that brings me to other assumptions of the Newtonian philosophy. But before going on to that I would ask you what significance we are to ascribe to Newton's absolute, uniformly flowing time? How can we measure it? In the last resource our measurement of time is a matter of observing certain astronomical phenomena. And Newton himself says that it is possible that no absolutely uniform motion exists in nature. But absolute time, he goes on to say, proceeds unaltered. How, then, can we know whether we are measuring absolute time? If all physical processes suddenly proceeded at double the rate they were going at before, no difference whatever would be perceptible. In fact, unless we assume Newton's absolute time, it would be meaningless to say they were going at double the rate.

Ord. Int. Per.: But if there is no absolute time, is not your choice of a standard of time quite arbitrary? I have heard it said that the earth is the standard clock. If this is so then its period of rotation must, by definition, be constant. But I have also heard that some astronomers have

held that the earth is slowing down. How can they know, or rather, how can that be?

Math. Phys.: Your question brings me to one of the directing principles in the creation of scientific theories. In describing phenomena we are guided by convenience and simplicity. I agree with you that if the earth were defined as the standard clock, then obviously it could not be said to be slowing down. But it is not so defined. Our notion of equal periods of time is deduced from a whole complex of astronomical phenomena, and to say that the earth is slowing down means that we find it more convenient to suppose this rather than to assume that certain other phenomena are behaving in an irregular and capricious manner. To introduce a little complexity into the motion of the earth is more convenient than to introduce a great deal of complexity elsewhere. You will find that not only science, but most sensible human thinking, proceeds on the same principle.

Phil.: You have two principles, then. Just now you rejected Newton's space because, though simple, it could not be detected, and now your

criterion in choosing your time standard is that it enables you to describe phenomena with the maximum amount of simplicity.

Math. Phys.: Precisely. We employ both principles. We wish to introduce no entities which cannot be defined in terms of physical processes, "observed," as some people say, and in constructing our theories we aim at making as few assumptions as possible — at being as simple as possible. These are, indeed, our two guiding principles.

Phil.: Very good. But I would like to point out that "simplicity" is a purely relative term, and that your theories are simple only in relation to the aspects of phenomena with which they are concerned. A scientific description of the universe can be a simple description only of its scientific aspects; it is not a description, simple or otherwise, of the universe.

Math. Phys.: I never said there was no *raison d'être* for philosophy or religion or anything else. I am quite aware of the limitations of science. We try to introduce order and coherence into a certain region of experience — ultimately, per-

haps, into the whole of experience, but in terms of certain entities and certain relations between those entities. Ours is one of many ways of ordering experience, and we would never dream of claiming that our picture of experience is exhaustive.

Phil.: Quite so. But it is as well to know what we are doing. We must realise that in discussing science we are discussing one of several ways of making a picture out of the data of experience. Certain fundamental entities are assumed — more or less arbitrarily — and experience is accounted for in terms of relations between these entities. Other ways of ordering experience start with different fundamental entities. For some purposes we prefer one picture and for other purposes another picture. It is doubtful whether the whole of experience will ever be comprised within any one of these pictures. It seems to me important that our scientific friend should be quite clear on this point. Scientific men have sometimes talked about science as if it is all we know and all we need to know. By all means let the scientific man proceed only from things that can be

"observed." But let him allow the mystic or the artist to proceed from things that can only be "felt." "Live and let live," should be the present day motto for science.

Math. Phys.: May I go on now?

Ord. Int. Per.: Did not Newton prove experimentally that absolute motion occurred — at least in the form of absolute rotation?

Math. Phys.: You are referring to his experiment with a pail of water. That and the experiment of Foucault's pendulum have often been taken to show that absolute rotation, at any rate, must be admitted. The answer given by the relativist is really the same for both, so we may confine our attention to the pail of water. Newton's experiment amounts to this: Suppose we take a pail of water and hang it up by a string. Now twist the pail of water round several times and then leave the string free to untwist itself. The pail rotates but, to begin with, the water does not share in this rotation. Presently, under the influence of friction against the sides of the pail, the water also begins to rotate, and finally pail and water rotate as a whole. At this stage

the surface of the water, which was before quite flat, is depressed in the middle. Now stop the pail. The water continues to rotate and to remain depressed in the middle until gradually, under the influence of friction, it comes to rest and its surface again becomes flat. Now let us tabulate what has happened. In the first case, before the water has taken up the motion, there is relative rotation between the pail and the water, but no perceptible physical change, that is, no depression of the water. In the second case, when there is no relative rotation of the water and the pail, there is a physical difference — the water is depressed. In the third case, when there is again relative rotation there is a physical difference — the water remains depressed. If we regard the rotation of the water merely as a relation to the pail we are led to the extraordinary conclusion that in one case this rotation makes no difference to the shape of the water and that in the other case it does make a difference. Yet if rotation is purely relative to the pail the cases are precisely similar. Newton concluded that the rotation could not be relative, but must be absolute.

Ord. Int. Per.: Well, after all, that does not follow. The water is rotating not only with respect to the pail, but with respect to a number of other bodies — the earth, for example, and the fixed stars. Newton did not experimentally obtain a case of absolute rotation since there were, in fact, many bodies in the universe with respect to which the water was rotating.

Math. Phys.: Precisely; and that is, in essence, the answer of the relativist. I think that what Newton did was to imagine this experiment being performed in a universe which contained nothing but the pail and the water. He was prone to this kind of abstraction, as we shall see when we come to discuss his laws of motion. It is, indeed, a very natural thing to do, and it is very difficult, as the history of science shows, to stick to our principle of dealing only with "observed" factors. Newton, I believe, imagined that if this experiment were made in a universe containing nothing but the pail and the water, the same phenomena would be observed. Our reply is that we have no reason to suppose anything of the

sort. We simply do not know what would happen, but we have no reason to believe that phenomena which take place in the universe as it is would be the same in an entirely different universe. To suppose that they would is to repeat the error of the early philosophers who legislated for the universe on the basis of supposed necessities of thought — such as Euclid's axioms — which we now know to be nothing of the kind. Newton's philosophy, by making appeal to unobservable factors is, as Einstein has said, partly metaphysical.

Phil.: Do you believe, then, that if the fixed stars were annihilated the earth would not be flattened at the poles?

Math. Phys.: I do not know. But I see no reason to suppose that, in such a case, the earth would be flattened at the poles. But I do not believe that we yet know all the difference that the presence of the fixed stars makes to phenomena. Very extraordinary things, of which we have no idea, might occur with the abolition of the fixed stars.

Phil.: Would not the experiment of Foucault's pendulum prove that the earth rotated, even if we were unable to observe the fixed stars?

Math. Phys.: It would prove that the earth rotated with respect to the plane of Foucault's pendulum. And it is an interesting fact, by the way, that the rotation of the earth, if accurately determined by Foucault's pendulum, is not quite the same as its rotation with respect to the fixed stars.[1]

Phil.: Well, I will admit that you are not compelled to believe in absolute rotation. But I am still in the dark as to how you deal with it on your theory.

Math. Phys.: That will come later. All that I want you to admit at present is that we have not yet found any reason to believe in the existence of absolute motion. We are now in a position to discuss critically the formal foundations of the Newtonian theory, his laws of motion. His first law states, you remember, that a body persists in its state of rest or of uniform motion in a straight line, except in so far as it is acted upon

[1] Eddington's *Mathematical Theory of Relativity,* p. 99.

by external forces. Now let us consider the assumptions implied by this statement. In the first place we must remember that the law is a deduction from experiment. Galileo was led to this law from the observed behaviour of bodies on the surface of the earth. Of course, no body on the surface of the earth is ever entirely free from the action of external forces, and therefore no experimentally controlled body can ever exactly fulfil the law. Nevertheless Newton, in enunciating this law as a general law of nature, supposed that if the effect of every force acting on the body were allowed for, it would be found that the body's departure from a state of rest or of uniform motion in a straight line was thereby accounted for. As in the discussion on absolute rotation, he is again making appeal to empty space.

Ord. Int. Per.: But is it not a fact that the Newtonian law is found to be obeyed.

Math. Phys.: Where?

Ord. Int. Per.: In the solar system, for example.

Math. Phys.: But the planets do not move in straight lines with uniform velocities.

Ord. Int. Per.: No; but that is because of the force of gravitation.

Math. Phys.: Exactly. The force of gravitation is introduced because the unconstrained motion of a body in a straight line with uniform velocity can nowhere be observed. Newton's law really amounts to the statement that in certain unobservable conditions, namely, at an infinite distance from all gravitating masses, a system of reference could be found consisting, say, of three mutually perpendicular axes, measured from which the motion of a body thrown into space would be a straight line described with uniform velocity. The statement cannot be put to the test.

Phil.: But is it not indirectly tested by the results of astronomical observation?

Math. Phys.: What is found by observation is this: That if we choose a set of axes of reference having their point of intersection in the sun, and if we assume the Newtonian law of gravitation, the calculated paths of the heavenly bodies, assuming the Newtonian laws of motion, agree very well, in nearly all cases, with the observed

paths. That the Newtonian scheme of celestial dynamics, taken as a whole, describes the observed phenomena extremely well goes without saying. Otherwise it would not have been accepted by scientific men for so long, and we should not now be discussing it. But I am discussing, at the moment, Newton's first law of motion, and pointing out that it refers to a condition of things which cannot be observed. Newton's scheme as a whole gives a very good picture of experience, but the manner in which the scheme is built up seems to us, nowadays, rather odd. In order to describe the observed happenings in the actual universe, Newton first tells us what would go on outside the universe. In order to make this statement relevant he then has to introduce an entity, the force of gravitation, to turn motions natural outside the universe into the motions which take place in the universe.

Ord. Int. Per.: You do not mean to deny that there is a force of gravitation?

Math. Phys.: I mean to say that the notion of a force of gravitation is made inevitable by accepting Newton's first law of motion. The

general concept of "force" long preceded, of course, the Newtonian theory. It is a generalisation from our own experience of muscular effort. We know that we can change the direction of motion of a body by hitting it, and the harder we hit it and the less heavy the body the more we change its motion. Newton's concepts were not arbitrary. They were the crystallisation of a great deal of experience. It was probably inevitable that his attempt to describe nature should be in terms of the concepts he actually chose. But the concepts have to be taken *en bloc*. We believe in Newton's force of gravitation because the observed motions of the planets, stones thrown into the air, and so on, are never straight lines described with uniform velocities, and we believe in his first law of motion because the universal force of gravitation explains why the motion described is not one that we observe. I merely wish to point out that direct evidence of the truth of the first law of motion is, on the Newtonian theory of the universe, unobtainable, since we can never observe a body under the action of no forces. Judged from the stand-

point of our principle of introducing none but "observed" factors, this method of building up a theory is undesirable.

Phil.: It seems to me that in your remarks you are trying to pursue two trains of thought at the same time. You are suggesting to us that the Newtonian entities, space, time, force, inertia, are not *necessary* concepts in an attempt to form a scientific picture of experience. This may be quite true. I understand that all these notions become something different in Einstein's theory. But you cannot make it an objection to Newton's theory that his concepts all hang together. Take, for instance, this omnipresent gravitational force which he finds in the universe. It is weaker in some places than in others, and the motions of bodies not otherwise influenced are such that as the gravitational force gets weaker they approach more and more nearly to straight lines described with uniform velocities. This happens in such a way that if there were *no* gravitational force, the motions would be *exactly* straight lines described with uniform velocities. Admitting his concept of "force," therefore, it is quite legitimate for

him to take a limiting case — the absence of force — even if it can never be observed.

Math. Phys.: You have not quite seen my point. I have already said that the Newtonian scheme, as a whole, gives a good picture of experience. We can, in practice, find a set of axes whose origin is in the sun, or, more accurately, in the centre of gravity of the solar system, measured from which Newton's first law of motion, combined with other dynamical laws, may be said to account, to a high degree of approximation, for the observed motions of bodies. I may mention that the set of axes obtained in this way are not ideally correct, but they are the best we can get empirically. But my real objection is this: Newton, in his first law, tells us what would happen to a body at an infinite distance from all other bodies. In doing so he ascribes a certain absolute property to this body. The property in virtue of which a body remains at rest or in uniform motion when acted on by no forces is called its "inertia." Newton's law says that this property is absolute. It is in no way dependent on the existence of other bodies.

It is to this point of view that I object. The property of inertia is abstracted from the observed behaviour of bodies, and such behaviour takes place in the universe as it is, that is, in the presence, amongst other things, of the fixed stars. Just as I see no reason to believe that we know what would happen to a rotating earth in the absence of the fixed stars, so, in the same circumstances, I do not know what would become of a body's inertia.

Ord. Int. Per.: Admitting, then, that the inertia of a body may depend on the existence of other bodies, Newton's laws do describe what actually goes on.

Math. Phys.: Assuming the whole apparatus of forces and so on we can empirically determine a set of axes of reference with respect to which the planets and other bodies very nearly describe the motions which can be calculated on the Newtonian scheme. There are discrepancies, of which the most interesting is connected with the motion of Mercury.

Ord. Int. Per.: What do you mean by axes of reference?

Math. Phys.: In order to determine the position of an event in space and in time we require to make four measurements, three of space and one of time. To make the time measurement we choose a clock of some kind, and to make the space measurements we choose a framework consisting usually of three rigid arms at right angles to one another and intersecting in a point. This point is called the origin. The floor and two adjacent walls of this room, for instance, meet in a point, a corner of the room. I can specify the position of any object in this room by giving its perpendicular distance from the floor and its perpendicular distances from the two adjacent walls. I must make three independent measurements to determine its spatial position. This is what is meant when we say that space has three dimensions. In order to describe the motion of anything, I have to give the positions it occupies at every instant. I measure its position in space by its distances from my axes of reference, and I determine the moment at which it occupies that position by my clock. It is now usual to take the whole thing, the clock used and the

spatial axes, and call it the system of reference.

Ord. Int. Per.: Then there can be any number of systems of reference?

Math. Phys.: Certainly. It is interesting to note that if Newton's laws of motion hold good in some system of reference, they will hold good in any system which is moving in a straight line with uniform velocity with respect to the first.

Phil.: The clocks must be keeping the same time, of course.

Math. Phys.: Yes, and the standards of length must remain unchanged. We shall see later what is involved in that assumption. I am assuming at the moment our ordinary Newtonian view of length and time. The Newtonian relativity I am talking about is something that can be deduced from the Newtonian equations of motion. Suppose we have a system of reference of the kind I have described, consisting of three spatial axes and a clock, and that we make measurements, from this system, of the motion of a body. Suppose we find that this body, measured from our system, is describing a straight line with uni-

form velocity. Take another system where the clock keeps time with the first one, and let the second system be moving, as measured from the first system, with uniform velocity in a straight line. Then the motion of the moving body, referred to this second system, will again be in a straight line and described with uniform velocity. A man standing on a railway platform, for example, may see an aeroplane flying in a straight line with uniform velocity. A passenger in a train which is itself travelling in a straight line with uniform velocity with respect to the man on the platform will find that the motion of the aeroplane, with respect to the train, is in a straight line with uniform velocity.

Ord. Int. Per.: But it will not be the same straight line and the same velocity as the one measured by the man on the platform.

Math. Phys.: No. To the man in the train the aeroplane may even appear at rest, if he is travelling in the same direction with the same velocity. The first law says, "rest or uniform motion in a straight line." Both cases are allowed for. What is invariant about the motion of such

a body, whichever one of the possible number of Newtonian systems it is referred to, is not its direction or magnitude. What is true for every one of these systems is that the motion will appear to be in a straight line and that its velocity will be uniform. This is not true of any other system. To a man sitting on a roundabout, for instance, the aeroplane will not appear to be describing a straight line with uniform velocity. Newton's first law of motion, therefore, holds only for a certain group of systems of reference. There are an infinite number of members of this group, but it is only for this group that Newton's law holds. His second law holds good for the same group of systems of reference, and gives us a magnitude which is the same for every member of the group, to wit, the force acting on a body. Newton's second law of motion states that the force acting on a body is equal to the mass of the body multiplied by its acceleration.

Phil.: Is this law to be taken as giving a method of measuring the force acting on a body or merely as a definition of the word force?

Math. Phys.: It is quite certain, I believe,

that Newton took "force" to be a concept with which every one was familiar, and enunciated this law to show how it could be precisely measured. The mass referred to is what is called the "inertial mass" of a body. Both concepts are derived from our own sensations of effort. We know that we can have two bodies of the same size, say a lump of lead and a lump of cork, and that it requires a greater effort to push one than to push the other. We say that a greater force is required in the one case than in the other, and we say that the body which is harder to push has a greater mass. We measure force by the amount of motion we can impart to a given mass in a given time. Before we can measure force precisely, therefore, we must be able to measure mass precisely.

Ord. Int. Per.: Cannot you do that by weighing the body?

Math. Phys.: At the present stage of the argument, certainly not. The weight of a body, according to Newton, is due to that universal force of gravitation he discovered. For a body weighed on the surface of the earth its gravita-

tional mass is a coefficient of attraction between that body and the earth. We must distinguish this from the coefficient of inertia, or inertial mass, which is a measure of the body's resistance to a change of uniform motion. Suppose I have two perfectly elastic bodies moving with velocities v_1 and v_2 respectively, and I wish to study what happens when they collide. After the collision let their velocities be u_1 and u_2 respectively. v_1 will not in general be equal to u_1 nor v_2 to u_2. Nor will $v_1 + v_2$ be equal to $u_1 + u_2$. But I find that I can choose two coefficients, m_1 and m_2 such that $m_1v_1 + m_2v_2 = m_1u_1 + m_2u_2$. Further, when I have found the m_1 for the first body and the m_2 for the second body I can take either of these and make it collide with a third body. I shall find the same m_1 that I found for the first body and the same m_2 that I found for the second body. In other words, I deduce the same coefficient for the same body whatever may be the coefficients of the other bodies it collides with. I call this coefficient the inertial mass of the body. Now suppose I take these bodies and perform an entirely different experiment. In

accordance with Newton's force of gravitation they attract one another and the attraction varies with the gravitational mass. I determine coefficients, characteristic of each body, as I did in the collision experiments. I call these M_1, M_2, and so on. What reason have I to suppose that these will be the same, or proportional to the m_1, m_2, and so on that I determined before?

Ord. Int. Per.: Can you make your point clear without using equations?

Math. Phys.: If you look at the equations again you will find them perfectly easy to understand. However, I will try to explain myself without using them. We find that a body resists endeavours to change its motion. This resistance varies with what Newton called the quantity of matter in the body or, as we say, its inertial mass. Now this property, according to Newton, is an absolute property of the body; matter happens to be that kind of thing. But besides this a body possesses, according to Newton, the entirely independent property of attracting every other body according to the law of gravitation. In the enunciation of this law figures a property of

the body called its gravitational mass, just as the electric charge figures in the law of attraction between two electrically charged bodies. Now what I want to emphasise is the extraordinary fact that, whatever substance the body may be made of, the ratio of its inertial to its gravitational mass is precisely the same as for a body made of any other substance. Newton's theory provides no explanation whatever of this coincidence, and yet it is so rigorously true and so universal that it must have a profound significance. As a matter of fact, it was by brooding on what this significance could be that Einstein was led to create his generalised theory of relativity; the meaning of this coincidence had escaped every other thinker up to his time.

Ord. Int. Per.: What is the proof that the inertial mass is always the same proportion of the gravitational mass?

Math. Phys.: Well, one simple but rather rough proof is that all bodies fall towards the earth with the same acceleration. The force pulling them is proportional to their gravitational masses. Since they all experience the same acceleration

the force must also be proportional to their inertial masses. Therefore the ratio of the gravitational to the inertial mass must be the same for every body. This holds good whatever may be the chemical or physical constitution of the body. The most precise measurements made, those of Eötvös, show that the difference between the two masses cannot be greater than five one hundred millionths.

Phil.: Very well. Let us see now, where we stand. You have shown us that there is no compelling reason to believe in absolute space or in absolute time, so far as science is concerned. In doing this you have insisted throughout on your criterion of measurement. You ask: How does one measure time? How does one measure space? I might point out that you are not really talking about space and time, but only about operations of measurement.

Math. Phys.: I am quite prepared to accept that. I am quite prepared to say that in talking about space and time I am talking about the behaviour of measuring appliances. But that is all that space and time can mean for science.

As a scientific man I have no interest in a space or time which cannot be identified, which cannot be distinguished from any other space and time. If you speak of a moment of absolute time or a point of absolute space I do not understand you until you tell me how I am to identify that moment or that point. Space and time, so far as I can see, consist of relations between events; they are not fundamental somethings within which events are situated.

Ord. Int. Per.: I confess that the abstract notions of space and time seem to me to be — well, rather abstract. Of absolute time I can really form no conception at all. I cannot see at all, for instance, why two swings of a pendulum should be said to take the same time except that to say so makes a lot of other things simpler. But it seems to me that, in the matter of space, you have a criterion. I understand that scientific men assert that there is an æther, a kind of elastic solid that fills all space, and that it can be shown that this æther is immovable. It seems to me, therefore, that the points of this æther always remain at the same points of space. Mo-

tion with respect to the æther, therefore, would do quite well as a specimen of absolute motion.

Math. Phys.: Quite well, as you say. If motion with respect to the æther could be demonstrated, scientific men would be quite content to waive philosophic objections, and to call it a case of absolute motion. But the interesting fact is that motion with respect to the æther cannot be demonstrated, and the implications of that discovery bring us to Einstein's restricted principle of relativity, which will be discussed in our next dialogue.

SECOND DIALOGUE

SPACE AND TIME MEASUREMENTS

Math. Phys.: I have already said that, on the Newtonian system, there is no way of distinguishing, by mechanical phenomena, between two systems which are in uniform motion with respect to one another. There is no measurable dynamical quantity which depends on absolute velocity. Acceleration, on the Newtonian system, is an absolute quantity. It has the same value from whatever system it be measured, provided the only difference between these systems is that they are moving with respect to one another in straight lines with uniform velocities. The mass of a body is also, on the Newtonian system, regarded as independent of its velocity. The product of these two quantities, therefore, the mass of a body and its acceleration, gives us a quantity, *force,* which is invariant for all Newtonian systems of reference. So far as dynamical experiments are

concerned, an observer cannot tell whether he is at rest or moving with uniform velocity in a straight line. The distinction is, in fact, scientifically meaningless, provided we are limited to dynamical experiments. That is a simple consequence of Newton's laws of motion. If Newton's equations ruled the whole of phenomena we should know that absolute velocity was a meaningless term.

Ord. Int. Per.: Are you now referring to absolute space or to the æther?

Math. Phys.: To either or both.

Phil.: But Newton's equations, I understand, do not cover the whole of physical science?

Math. Phys.: No. Strenuous efforts have been made to enable them to do so, but the attempt has had to be given up. The phenomena of light obey a set of equations which cannot be reduced to those of Newton. These equations were given by Maxwell. Now the interesting fact arises that these equations are not invariant when referred to different Newtonian systems of reference.

Ord. Int. Per.: That is very interesting. The

phenomena of light, therefore, would be different when referred to two systems in uniform motion with respect to one another?

Math. Phys.: It would seem so from what I have said. But the fact is that systems in uniform motion with respect to one another are not connected by the formulæ given by Newton. Let us choose a system of reference and denote the three space measurements and the time measurement by x, y, z, and t respectively. Let us take a second system moving with a uniform velocity v with respect to the first system in the direction of the x axis. Let them coincide initially, and let the clocks in both systems start measuring from zero initially. Then Newton says that the corresponding space and time measurements in the second system, which we will call x', y', z', and t', are connected with those in the first system by the equations $x' = x - vt$; $y' = y$; $z' = z$; $t' = t$. We shall see that this statement is untrue.

Ord. Int. Per.: I see. Then you mean that Maxwell's equations do hold good for systems in uniform motion with respect to one another,

but that they do not transform according to Newton's rules because those rules are inaccurate?

Math. Phys.: Precisely.

Phil.: Then, in that case, Newton's laws of motion are not invariant for systems moving with uniform velocity with respect to one another?

Math. Phys.: They are not. When we have found the true formulæ connecting such systems we shall find that we have to replace Newton's laws by others in order that the laws of dynamics may remain invariable for such systems.

Phil.: Is not that a rather arbitrary proceeding? Are you prepared to alter the laws of nature because you are determined never to discover a case of absolute motion?

Math. Phys.: Certainly not. You will find that all experiment confirms the altered laws. I enunciate principles, but only because they are justified by experiment. The principle I am about to discuss with you now is Einstein's restricted principle of relativity and may be enunciated thus: the laws of physical phenomena are the same when referred to any one of a group

of systems of reference which are moving with respect to one another in straight lines with uniform velocities.

Phil.: This is an extension of Newton's principle of relativity to all phenomena.

Math. Phys.: Exactly. And it is a very extraordinary extension, as you will see if you reflect for a moment on the phenomena of the propagation of light. Light has long been supposed, as you know, to consist in wave disturbances in a universal medium called the æther. The chief reasons for supposing that this medium exists is that light takes time to travel, and that its velocity is independent of the velocity of the source that emits it. In these respects it behaves like the waves that spread out when a stone is thrown into a pond. The velocity of the waves does not depend on the velocity of the stone; it depends wholly on the characteristics of the pond. If light travelled like bullets shot out of a gun its speed would vary with the speed of the source. But light always has the same velocity. Now let us imagine the æther as a gigantic pond through which the earth is moving. On the earth

I fix up an apparatus consisting of two equal arms at right angles. At their point of intersection I have a source of light and at each of their ends is a mirror. I point one arm in the direction of the earth's motion through the æther. The other arm is, of course, at right angles to it. At a given moment I unveil the source of light. A ray passes to each mirror and is reflected back again. Will each ray take the same time to make the double journey?

Ord. Int. Per.: I should have to work that out.

Math. Phys.: Well, but suppose you are a swimmer in a stream. You make two journeys. On one of them you swim as far up stream as the stream is wide and then you swim back again. On the second journey you swim across stream to the opposite point on the bank and back again. Which journey takes the longer time?

Ord. Int. Per.: I should say it would take longer to swim up stream and down again.

Math. Phys.: You are perfectly right. I need not bother to work it out for you now. You should do it for yourself some time. It is perfectly simple. Now this case is precisely analo-

gous to the earth moving through the æther. The ray of light is our swimmer, and it is evidently the same thing whether we imagine the earth moving through the æther, or the æther flowing past the earth. Well, this experiment has been performed, with the astounding result that there is *no* difference in the times taken by the two rays of light.

Phil.: I find your description of this experiment very obscure. You enunciate as a principle that you cannot tell uniform motion from rest, and then you tell me that you place an arm of an apparatus in the direction of motion of the earth through the æther.

Math. Phys.: I was waiting for that comment. It is perfectly justified. But the point is that *if* there is an immobile æther then the earth, at some period of the year, is moving with respect to it, for the simple reason that as the earth describes an orbit round the sun its direction of motion turns through a complete circle in the course of the year. If, therefore, it should happen to be immobile with respect to the æther on the first of January it certainly cannot be so on

the first of July. Now this famous experiment, the Michelson-Morley experiment, was made at all seasons of the year, and the arms rotated through all angles. In every case the result was null. It proved conclusively that relative motion between the earth and the æther could not be detected.

Ord. Int. Per.: But surely there are other possibilities. It might be, for instance, that the æther in the neighbourhood of the earth is carried along with it in its movement.

Math. Phys.: No. That hypothesis has been tried. The astronomical phenomenon of aberration is against it.

Ord. Int. Per.: Well, perhaps, after all, the velocity of light is influenced by the motion of its source. Perhaps the light is fired out quicker in the direction of the earth's motion.

Math. Phys.: No, that theory will not do either. I can tell you one very neat argument against it. In a double star, as you know, we practically have one star revolving round another, if one is much heavier than the other. Consider the revolving star. If the velocity of light varies

with the velocity of the source then the light sent to us by the star at different points of its path will have different velocities. Even if the differences in velocity are small, the journey to the earth takes so long that the differences in the times of arrival of the rays will be appreciable. We should get fast rays, sent out later, arriving simultaneously with slower rays, sent out earlier. As a result we ought to see the star in several positions on its orbit at the same time. Such a phenomenon has never been observed.

Phil.: Well, then, how do you explain the Michelson-Morley experiment? It appears that you must assume an æther for your science of light, and yet you can never detect your motion with respect to it.

Math. Phys.: Well, the theory put forward by two very able scientific men was that every body contracts in the direction of its motion. If a certain ratio is assumed for this contraction we can make the up and down journey along one arm take the same time as the across and back journey along the other. This contraction, of course, affects all bodies in exactly the same way.

It does not matter what materials the arms are made of.

Ord. Int. Per.: Why should this contraction take place?

Math. Phys.: Nobody knows.

Ord. Int. Per.: It was invented, then, simply to explain the null result of the Michelson-Morley experiment? There is no other reason for believing in it?

Math. Phys.: Precisely. The assumed contraction can be shown to be compatible with the electro-dynamic equations, but no physical reason founded on the interaction between, for instance, electrons and the æther, can be given. But we shall see that the contraction does occur. It is a consequence, however, not of some action of the æther, but of our way of measuring space and time. We must remember two principles. One is that phenomena take place in the same way whether we suppose ourselves to be in a system in uniform translatory motion or at rest. The other is that the velocity of light in vacuo is constant. We shall begin by considering what we mean by *simultaneity.*

Phil.: I should like first to point out that the two principles you have just enunciated are of very different kinds. The assumption that the velocity of light in vacuo is constant is, I take it, the result of experiment. Further experiments could conceivably show that it is not always constant. You do not regard it as a *necessary* preliminary, in your attempt to give coherence and order to natural phenomena, to assume that the velocity of light must be constant.

Math. Phys.: You are quite right. Later on we shall give up the assumption, but not in such a way as to invalidate the results we reach by making it. We may consider it at present as an extremely well attested experimental fact.

Phil.: Your other principle, however, which amounts to saying that rest and uniform motion are indistinguishable is, although confirmed by experiment so far as the æther is concerned, primarily the enunciation of a certain *form,* a kind of framework, within which natural phenomena must be arranged if they are to be intelligible to you. You have decided that rest and uniform motion, except in their relative meanings,

are not terms which have any physical significance. You regard them, I take it, as concepts which are useless or misleading if used to build up physical science. When you say that natural phenomena do not enable us to distinguish between systems which are in uniform translatory motion with respect to one another, you do not mean that they *might* do so, but as an ascertained fact do not; you mean that there can be nothing in nature corresponding to a distinction you regard as meaningless.

Math. Phys.: Yes, that is more or less what I mean. I cannot imagine, for instance, that the laws of a natural process depend on whether it is taking place on my right or on my left, in front of me or behind me. Laws into whose enunciation such considerations entered would obviously not be true laws. It is inconceivable that fundamental laws of nature depend upon the observer to that extent. And I put the difference between uniformly moving systems in the same category. You may call this, if you like, a form imposed upon the laws of nature by my own mind. I admit that you cannot make a science without

principles, and those principles must express the constitution of our own minds.

Phil.: But if you make it a principle that the laws of nature shall be the same when referred to systems in uniform translatory motion with respect to one another, why do you not go further, and say that these laws must be the same for *all* systems of reference?

Math. Phys.: That is just what I propose to do. When I come to discuss Einstein's generalised theory of relativity you will find that we are satisfied only with those laws which are invariant for all systems of reference. But we must proceed by stages, and we will first deal with the case of uniform translatory motion. There is no logical necessity for this, but it is psychologically desirable.

Ord. Int. Per.: You were going to begin by discussing the notion of simultaneity, were you not?

Math. Phys.: Yes. And here we must remember, in accordance with what I have said before, that no notion of simultaneity is of any use to us which cannot be defined in terms of physical

processes, or which is not a direct perception. I introduce that last clause because I am going to assume that the simultaneity of two events in one's close neighbourhood is directly perceived. If, for instance, I say that a flash of light in my neighbourhood occurred at a moment t, I mean that the hands of my clock coincided with certain marks on the dial, indicating the time t, simultaneously with the occurrence of the flash. Simultaneity of this sort I do not propose to define further. If I say, for instance, that the rays of light from the clock when it indicates t, and from the flash of light, arrive at the retina of my eye at the same time, I merely transfer the simultaneity to an occurrence at the retina of my eye. I shall assume, therefore, that you know just what I mean by simultaneity in this case. But when we deal with events at a considerable distance apart a definition is required. Suppose I have an observer A provided with a clock, and, at some distance away, another observer B also provided with a clock. How can A know that an event occurs in his neighbourhood

simultaneously with another event in *B*'s neighbourhood?

Ord. Int. Per.: *A* can see at what time his event occurs and compare it with the time, marked by *B*'s clock, when *B*'s event occurs. If these times are the same the events were simultaneous, provided, of course, that the clocks are keeping the same time.

Math. Phys.: Precisely. But how do you know that the clocks are keeping the same time?

Ord. Int. Per.: Well, the clocks may have been together to begin with, and adjusted to keep the same time. Then *B* takes his clock with him to his new station.

Math. Phys.: But how do you know that the movement will not affect the rate of the clock? As a matter of fact, it will do so, unless the clock be moved infinitely slowly. In that case *B* would never get to his new station. To get a practical definition we must assume that *A* and *B* are already in position and frame a rule that they can apply. The obvious method is to employ light-signals. When *A*'s clock marks a

time t_1, let him flash a signal to B. It reaches B when B's clock marks t_2. B immediately flashes back a signal to A which reaches him when A's clock marks t_3. If now $t_2 - t_1 = t_3 - t_2$ the clocks are said to be synchronised, and A and B assume that the instant $\frac{1}{2}(t_3 - t_1)$ marked by A's clock is simultaneous with the instant t_2 marked by B's clock.

Phil.: They have assumed that the light has taken the same time to go from A to B as to go from B to A.

Math. Phys.: Exactly. They must do so to get a definition of simultaneity. They must assume that the forward velocity of light along any line is equal to its backward velocity.

Ord. Int. Per.: Is there any experimental evidence for that assumption?

Math. Phys.: No. All experimental methods determine the average to and fro velocity of light. But the assumption conflicts with no observations, and the complications introduced by not assuming it appear entirely gratuitous, if not unintelligible. Now let us assume a system, say a long rod, with a number of clocks arranged

along it. Let all these clocks be synchronised by the method I have just described. Then to an observer at rest on this rod all these clocks are marking the same time. Now let there be another rod, parallel with the first, and of length l. We assume, to begin with, that these rods are at rest with respect to one another. In that case the length l, measured by an observer on the second rod, would agree with the length measured by an observer on the first rod, who measured the distance between the points on his own rod adjacent to the beginning and end of the second rod. Also, on the second rod, let there be two clocks, one at the beginning of the rod and the other at its end. Let these two clocks agree with the clocks to which they are adjacent on the first rod. The observers on both rods agree in their measurements both of length and of time. But now suppose that the second rod is moving along the first rod with a velocity v with respect to the first rod. We shall imagine that the clocks on the second rod are so arranged that at any moment they mark the same time as the clocks adjacent to them on the first rod, that

is, the clocks on the second rod obey the time system of the first rod. At the moment when the beginning of the moving rod coincides with the beginning of the first rod let a light signal be sent out. Now we shall not assume that the length of the moving rod, measured from the stationary system, remains l. Let its length measured from the stationary system be r. Then, to reach the end of the moving rod, the light, measured from the stationary rod, takes a time $\frac{r}{c-v}$, where c is the velocity of light — the same, you remember, for all observers. When the light reaches the end of the moving rod let it be immediately reflected to the beginning of the rod. For this journey it takes a time $\frac{r}{c+v}$ measured from the stationary rod. Whatever r may be, these two times are not equal. To observers on the moving rod, therefore, the light takes longer, according to the clocks they have been provided with, to go from the beginning of the rod to the end than to go from the end to the beginning. To these observers, therefore, the clocks are not

synchronised. Since, according to our principle, they have every right to say that they are stationary and that the other system is moving, they will reject the time system indicated by their clocks and proceed to synchronise them in the ordinary way. The time measurements adopted by observers moving with uniform translatory motion with respect to one another are different.

Ord. Int. Per.: This is a little difficult to follow. In your formula for the time taken by the light to go from the beginning of the moving rod to the end you make the velocity of the light signal $c - v$.

Math. Phys.: Yes. To allow for the velocity v of the moving rod. The light is travelling with velocity c for the stationary observers, and the moving rod is travelling with velocity v. The light overtakes the end of the rod, therefore, with velocity $c - v$.

Ord. Int. Per.: But you say the moving observers think the light is overtaking the end of the rod with velocity c.

Math. Phys.: Yes; because they have no

reason to suppose that the end of their rod is running away from the light. They may assume they are at rest.

Ord. Int. Per.: But surely the light cannot be overtaking the end of the rod at two different rates at one and the same time.

Math. Phys.: Yes, it can. The observers on the moving rod can measure the velocity of the light signal and they find it is *c*. Remember our principle : The velocity of light is the same when measured by observers in systems which are in uniform translatory motion with respect to one another. You are beginning to see how remarkable an experiment the Michelson-Morley experiment was. But I have already suggested the explanation to you. I have pointed out that the moving observers, in order to synchronise their clocks, would adopt a different *time* from the stationary observers. They would also, as a matter of fact, adopt a different measure of length, as you can see quite easily if you reflect that the length of a moving rod is the distance between the marks on a stationary scale occupied by the beginning and end of the rod at the *same*

instant. Since what is simultaneous for the stationary observer is not simultaneous in the time system of the moving observer, it follows that their measures of length will be different. Now the velocity of light is — a velocity; that is, it is the ratio of length and time. Since we have seen that different observers have different measurements of space and time it may be that these differences are just of the kind to give the same value for the velocity of light, although the systems are moving with respect to one another. As a matter of fact, that is exactly what happens. If x, y, z, t, are the space and time co-ordinates used by one observer, and x', y', z', t', are the space and time co-ordinates used by an observer travelling with a uniform velocity v with respect to the first observer, then the condition that they both find the same velocity for light enables us to deduce the equations connecting the space and time measurements of one observer with those of the other. Let the velocity v be in the direction of the x measurements, and suppose the systems of reference initially coincided. Then we have the following relations : —

$$x' = \beta(x - vt);\ y' = y;\ z' = z;\ t' = \beta\left(t - \frac{vx}{c^2}\right)$$

$$\text{where } \beta = \frac{1}{\sqrt{\left(1 - \frac{v^2}{c^2}\right)}}$$

Now these equations are very simple, and they are full of extraordinary and important information. Notice first that $\frac{v^2}{c^2}$ is usually a very small quantity, for the velocity of one system with respect to another is very rarely anything approaching the velocity of light. In fact, it is only electrons which move at speeds comparable with that of light. Since $\frac{v^2}{c^2}$ is usually extremely small it follows that $1 - \frac{v^2}{c^2}$ is usually nearly equal to 1. It follows that β is usually nearly equal to 1. Suppose the velocity of light, instead of being c, that is, 300,000 kilometres per second, were infinite. Then $\frac{v^2}{c^2}$ would be zero and β would be 1. Also $\frac{vx}{c^2}$ would be zero, so that the expression for t' would reduce to $t' = t$. The time measurements would be the same. Also, x'

would reduce to $x' = x - vt$. Our equations would become, therefore, —

$$x' = x - vt;\ y' = y;\ z' = z;\ t' = t.$$

If you compare these with the equations I gave you before you will see that these are the equations by which Newton links up two systems moving in uniform translatory motion with respect to one another. Newton's equations, therefore, would be correct if the velocity of light were infinite. As things are, the Newtonian equations are very nearly correct for all ordinary relative velocities. The fact, therefore, that the Newtonian equations have been verified by observation to a very high degree of approximation is not against our theory. It is only when very high velocities occur that the difference between the two sets of equations becomes appreciable. I may mention that moving electrons, when the velocity is very high, obey Einstein's equations and not Newton's. Let us look at our equations a little more closely. We have assumed that the x', y', z', t' system is moving with velocity v with respect to the x, y, z, t sys-

tem. It is not a *necessary* consequence, *à priori,* that the velocity of the x system with respect to the x' system is $-v$. But it turns out that this is so. If we solve for x and t in terms of x' and t' from the equations, we obtain the set:—

$$x = \beta(x' + vt');\ y = y';\ z = z';\ t = \beta\left(t' + \frac{vx'}{c^2}\right)$$

The only difference, you see, is that the v has changed sign, indicating that the velocity must be taken in the opposite direction from its former one. We also see that the length l of the moving rod is, measured from the stationary system, equal to $\frac{l}{\beta}$. That is, the length of a rod in uniform motion, measured from the system with respect to which it is moving, is contracted in the proportion 1: $\frac{1}{\sqrt{1 - \frac{v^2}{c^2}}}$

This is the same contraction that was proposed by Fitzgerald and Lorentz to explain the Michelson-Morley experiment.

Phil.: But this contraction is, it appears, a consequence of the fact that different observers

employ different space and time measurements. Has anything really happened to the rod?

Math. Phys.: No more than anything happens to a penny when from one point of view it is a circle and from another point of view is an ellipse. Length is not an absolute property of the rod. It is a relation between the rod and an observer. The relation differs for different observers. The distance between two events varies with different observers. Similarly, the time between different events varies for different observers. Suppose we have a clock at rest on the moving system and it beats out a time T seconds. Then, judged by the stationary system, a time βT seconds has elapsed, that is, something more than T seconds. Judged from the stationary system, therefore, the moving clock is going slow.

Phil.: Let us see at what point of the argument we have arrived. Suppose two events take place — say, two flashes of light. I will call them events A and B. To me, we will suppose, they occur at different places and at different times. I judge that B is later than A. Now you tell me that an observer could be imagined

to whom A and B were simultaneous events, or to whom A was later than B. Further, an observer might be found for whom A and B occurred at the same place or to whom, although A and B remained spatially separated, yet their distance apart would be different from my measurement.

Math. Phys.: Yes, in general that is so.

Phil.: Then what knowledge have you of these events which is not merely relative to the observer? How can a science, a way of ordering phenomena independent of any particular observer, be constructed?

Math. Phys.: It is because there *is* a relation between the events which is the same for all observers. In the first place, let me point out that every observer regards the events of nature as forming a four-fold order. Each observer makes three spatial and one temporal measurement for each event. Four quantities have to be assigned in order that an event may be fully specified in the space-time frame adopted, whatever that space-time frame may be. Now suppose that in some frame of reference we have an

event whose spatial measurements are x_1, y_1, z_1, and whose temporal measurement is t_1. From the same frame of reference let the measurements of a second event be x_2, y_2, z_2, t_2. Now form the expression —

$$(x_2 - x_1)^2 + (y_2 - y_1)^2 + (z_2 - z_1)^2 - c^2(t_2 - t_1)^2$$

and call this expression s^2. If $t_2 = t_1$, that is, if the events occur simultaneously according to the time measurement of this system, then —

$$s^2 = (x_2 - x_1)^2 + (y_2 - y_1)^2 + (z_2 - z_1)^2,$$

and this is the expression for the square of the distance between the events. In this case, therefore, s is the distance between the events. But, in general, s involves both distance and time. It is a *spatio-temporal relation.* Now let the measurements for the same two events in another system of reference be x'_1, y'_1, z'_1, t'_1, and x'_2, y'_2, z'_2, t'_2. Form the corresponding expression:—

$$(x'_2 - x'_1)^2 + (y'_2 - y'_1)^2 + (z'_2 - z'_1)^2 - c^2(t'_2 - t'_1)^2 = s'^2$$

You will find that $s' = s$. *The spatio-temporal relation represented by s is the same for all observers who are in uniform translatory motion*

with respect to one another. This invariant quantity is called the *interval.* It is more fundamental than either distance or time-lapse, and it is a combination of both. In order to simplify the expression and to make it more convenient I shall assume that we choose our units of space and time so that the velocity of light is unity. If we choose the kilometre as our unit of length, and the second as our unit of time, then c, the velocity of light, is 300,000. But if, instead of the second, I choose as my unit of time 1/300,000 of a second, the velocity of light is unity, that is, $c = 1$. Besides doing this, I shall alter the sign of s^2. Thus we get the formula —

$$(t_2 - t_1)^2 - (x_2 - x_1)^2 - (y_2 - y_1)^2 - (z_2 - z_1)^2 = s^2.$$

s^2 will now usually turn out to be a positive quantity, which is convenient. In order to illustrate the meaning of the interval, s, we may write it in a different way. Suppose that a particle is moving with velocity v with respect to some system of reference. Let it be at the point x_1, y_1, z_1 at time t_1, and at the point x_2, y_2, z_2 at time t_2. Then the square of the distance it has travelled divided by the square of the

time it has taken to do so equals the square of the velocity, that is, —

$$\frac{(x_2 - x_1)^2 + (y_2 - y_1)^2 + (z_2 - z_1)^2}{(t_2 - t_1)^2} = v^2,$$

and therefore

$$(x_2 - x_1)^2 + (y_2 - y_1)^2 + (z_2 - z_1)^2 = v^2(t_2 - t_1)^2.$$

So the expression for s^2 may be written —

$$(t_2 - t_1)^2(1 - v^2) = s^2, \text{ or } (t_2 - t_1)\sqrt{(1 - v^2)} = s.$$

If the velocity of the particle is equal to the velocity of light, that is, to unity, then $s = 0$. If light be emitted from a source at A at a time t_1, and strikes an object at B at a time t_2, the *interval* between these events is zero, although, of course, neither their spatial nor time separations are zero.

Ord. Int. Per.: Why should s be the same for all observers?

Math. Phys.: We do not know. We can merely say that the world happens to be that kind of world. The fact that s is constant for different observers is an experimental fact. We may take it as indicating the kind of four dimensional geometry that must be applied to the

world. Consider, for instance, a plane, such as the surface of this blackboard. I have two points on it. Each point requires two spatial measurements to specify its position. For the first point let these measurements be x_1, y_1, and for the second point x_2, y_2. Now the distance between these two points, as you know from Euclid, is —

$$\sqrt{(x_2 - x_1)^2 + (y_2 - y_1)^2}.$$

If I have a point in space, say the position of the tip of the electric light bulb in this room, I must give three spatial measurements to determine its position. Suppose I have two such points and let their measurements be x_1, y_1, z_1 and x_2, y_2, z_2. Then the distance between them is given by —

$$\sqrt{(x_2 - x_1)^2 + (y_2 - y_1)^2 + (z_2 - z_1)^2}.$$

You see that this expression is precisely analogous to the one for two dimensions. In a four dimensional space the distance between two points would be —

$$\sqrt{(x_2 - x_1)^2 + (y_2 - y_1)^2 + (z_2 - z_1)^2 + (t_2 - t_1)^2}$$

when t represents the fourth dimension. If, now, we take space and time together as forming

a four-dimensional continuum, it might be thought that the above expression would represent something important. But, as it happens, it does not. To get an important quantity the time distance and the space distance must be given opposite signs. This is an *experimental* fact, a peculiarity of the world we live in. The change of sign means that the four dimensional continuum that each observer splits up into space and time is not a Euclidean continuum. We may call it semi-Euclidean or hyperbolic.

Phil.: When you speak of a four-dimensional continuum are you not referring merely to a mathematical device for treating phenomena conveniently?

Math. Phys.: No, I mean more than that. The world is really four-dimensional. It is our division of it into a three-dimensional space and a one-dimensional time which is arbitrary, in the sense that this division varies for each observer. That we are under a *psychological* necessity to split up nature in this way I fully admit. But there is no objective counterpart to this division that we make.

Phil.: You must agree, then, that space and time are subjective.

Math. Phys.: Certainly. The one objective something that we have discovered so far is the *interval,* a spatio-temporal relation which different observers split up differently into space and time components.

Phil.: If space and time are as arbitrary as you suppose, what becomes of the notion of causality? We will suppose that a declaration of war, for instance, is followed by a battle. I, as an observer, see that the battle follows the declaration. Do you assert that there could be an observer to whom the battle preceded the declaration?

Math. Phys.: Not at all. Space and time divisions of the interval between two events are not as arbitrary as that. We shall see later on that the velocity of light is a critical velocity, and that no particle of matter nor any form of radiant energy can have a velocity greater than that of light. In the history of any material particle or of, say, a human being, there is an absolute succession of events — an absolute past

and an absolute future. No observer could make the declaration follow the battle although, if soldiers could move with velocities greater than that of light, he could do so. A man's death cannot precede his birth for any conceivable observer. If, in my frame of reference, an event at P_2 could be reached from P_1 with a velocity less than that of light, then P_2 is in the future of P_1 for every observer. I will go on to discuss the critical nature of the velocity of light in our next dialogue, but before doing so I should like to point out an interesting consequence of the equations we have arrived at. It might occur to you that the velocity of light could easily be exceeded for some observer. For suppose I have a particle moving with nine-tenths of the velocity of light with respect to an observer who is himself moving in the same direction with nine-tenths of the velocity of light relative to a second observer. You might think that the velocity of the particle, relative to the second observer, would then be greater than the velocity of light. But the equations show that this is not so. The space and time measurements vary in such a way that no

succession of velocities can add up to more than the velocity of light. The algebra is quite simple. Let the velocity of the particle relative to the first observer be u. Then, at two successive instants, t'_1 and t'_2, the particle is at distances x'_1 and x'_2, where —

$$\frac{x'_2 - x'_1}{t'_2 - t'_1} = u$$

Let this observer be moving with velocity v with respect to the second observer. Then, from our equations for the relations between their measurements the distance $x'_2 - x'_1$, expressed in the measurements of the second observer, becomes —

$$\beta(x'_2 - x'_1) + \beta v(t'_2 - t'_1).$$

This quantity is equal to the $x_2 - x_1$ of the second observer. Similarly, the time lapse $t_2 - t_1$, as measured by the second observer, is equal to

$$\beta(t'_2 - t'_1) + \beta \frac{v}{c^2}(x'_2 - x'_1).$$

Now the velocity of the particle, measured by the second observer, is equal to the distance it has described, that is, $x_2 - x_1$, divided by the time it has taken to do it, that is, $t_2 - t_1$. But —

$$\frac{x_2 - x_1}{t_2 - t_1} = \frac{\beta(x'_2 - x'_1) + \beta v(t'_2 - t'_1)}{\beta(t'_2 - t'_1) + \beta \frac{v}{c^2}(x'_2 - x'_1)}$$

If we divide both the numerator and denominator of this expression by $t'_2 - t'_1$ we get, remembering that —

$$\frac{x'_2 - x'_1}{t'_2 - t'_1} = u, \qquad \frac{\beta u + \beta v}{\beta + \beta \frac{vu}{c^2}} = \frac{u + v}{1 + \frac{vu}{c^2}}$$

Now suppose $u = \cdot 9c$ and also $v = \cdot 9c$. This expression becomes

$$\frac{1\cdot 8c}{1 + \frac{\cdot 81c^2}{c^2}} = \frac{1\cdot 8}{1\cdot 81}c$$

and this is less than c. Even if the first observer were measuring the velocity of a ray of light, which is c, and were himself moving in the same direction with the velocity of light with respect to the second observer, the velocity of the ray, measured by the second observer, would still be c. For the velocity would be, from our formula

$$\frac{c + c}{1 + \frac{c^2}{c^2}} = c.$$

This formula, as you see, is an extraordinary one. On Newton's theory velocities are merely added together and their sum may exceed any given quantity. On our theory velocities are not merely additive, and, however many be taken together, their sum can never exceed the critical velocity *c*.

THIRD DIALOGUE

ABSOLUTE AND RELATIVE RELATIONS

Phil.: Before going on to further technical consequences of the theory you are expounding to us, I should like to know precisely what this theory professes to accomplish. You have agreed with me that science is a method of ordering the data of experience. Newton began by prescribing certain fundamental entities in terms of which phenomena were to be ordered. You have described to us the way in which new entities have been substituted for these. Instead of distances between points of absolute space and moments of absolute time we have a spatio-temporal relation called the *interval.* Matter also, on the new theory, is not, I understand, fundamental. But besides primary entities science prescribes certain formative principles in obedience to which its attempt to order nature proceeds. For instance, it contemplates all happenings from the point of view of what I may call their measurability. The

aim of physics, it appears, is to assign four numbers, x, y, z, t to every event. These numbers have no "absolute" significance; in terms of them experience is ordered in a definite but arbitrary manner. Everything is "explained" or described as relations between numbers. It follows that whatever reality or absolute you arrive at is conditioned by your method. It is the outcome of your principles as well as of your experience. In arriving at the "interval," for instance, you find that space and time are not essentially distinct. So far as they enter into your scheme of ordering experience they do not enter as distinct entities. But you have not thereby exhausted the content of space and time, as they exist in experience. You are concerned with a part only of the total wealth of perceptions which constitute what we call space and time. If, therefore, you find that space and time are "merged in one" you have shown only that a distinction between them is unnecessary for the ordering of phenomena in accordance with the formative principles you have adopted. It is conceivable that other formative principles would lead to a description of nature

where the distinction was preserved, and where the space and time discussed consisted of a different group of perceptions.

Math. Phys.: I think that is quite true. When I speak of an " absolute " or of " reality " I mean something that remains the same for all observers who are trying to order experience in the same way as myself. The formative principles adopted in science are not the only principles open to man. It is true that every way of ordering experience is a limited way, and, in this sense, every reality that is reached is a relative reality. This applies as much to the mystic as to the man of science. It may be that the function of philosophy is to reach a reality which is the same for all modes of ordering experience. I cannot conceive what it could be, and I am not sure that such an aim is intelligible.

Phil.: I do not think it is intelligible. An ultimate reality, in that sense, is not even, I believe, a will-of-the-wisp.

Ord. Int. Per.: Let us return, then, to the consideration of the reality that science has reached so far.

Math. Phys.: I must first remind you that the object of science is to *unify* phenomena. It is true that there are still men of science, dominated by the Victorian tradition, who think the aim of science is to give a mechanistic explanation of phenomena. But the actual history of recent scientific development shows that this is not the case. The object of science is to include all phenomena within a scheme which makes a minimum number of assumptions. It is not bound to seek for an explanation in terms of the properties of matter any more than it is bound — as it was at one time supposed to be — to seek for an explanation in terms of the attributes of God. You will find that the theory of relativity takes us into a region where the fundamental concepts are quite different from those of inertia, rigidity, and the like. We have already arrived at the interval relation, and we shall see later that, in a more general form, this is a fundamental concept of the new theory, a concept from which the other concepts in physics may be derived.

Ord. Int. Per.: Is the interval the only meas-

urable quantity which remains the same for all observers?

Math. Phys.: By no means. An electric charge, for instance, has the same numerical value for all observers. But some other quantities, as volume, mass, density, and even temperature, are given different values by different observers. That a volume has different values for different observers is obvious since, referred to an observer for whom it is in motion, it contracts in the direction of its motion while its other dimensions remain unchanged. But the mass of a body also varies with the observer. In the experiment with the colliding billiard balls that I described to you in our first dialogue the values of the masses found by one observer will not be the same as those found by an observer moving relatively to the first. If we call m the mass of a body as measured by an observer at rest with respect to it, then its mass M as measured by an observer for whom it moves with velocity v, is —

$$M = \frac{m}{\sqrt{(1 - v^2)}}$$

taking units such that the velocity of light is unity. If v is small we may put approximately $\frac{1}{\sqrt{(1-v^2)}} = 1 + \frac{1}{2}v^2$, so that M becomes $m + \frac{1}{2}mv^2$. The term $\frac{1}{2}mv^2$ is the usual expression for the energy of motion of the body. Its mass, in our units, is increased by this amount. This suggests that energy and mass are convertible terms. They are two ways of measuring the same property of the body. We may suppose that m, the mass at rest, represents the concealed energy resident in the body. This energy is enormous. In ordinary units it is mc^2, where m is the mass of the body, and c the velocity of light. The energy associated with one gram of matter is 9×10^{20} ergs.

Phil.: Since the mass of a body varies with its velocity, what becomes of the law of the conservation of mass?

Math. Phys.: It is merged into the more general law which asserts the conservation of mass and of energy. If a body be heated its energy is increased. Therefore, on our present

theory, its mass is increased. But there is here no violation of the law of conservation, since the radiant heat energy which has been communicated to the body has mass, and it is this mass which accounts for the increase in mass of the body. Similarly, if a body radiates energy it loses mass, but the mass is carried away by the radiant energy.

Phil.: A body which is radiating energy, therefore, has no very precisely defined boundaries. Part of its mass, at a given moment, exists in the space around it, so that the body must be regarded as more or less diffused through the surrounding space.

Math. Phys.: That is quite true. But the constitution of matter may quite justly be called atomistic. Although matter, in the form of radiant energy, pervades the whole universe, it is yet true that incomparably the greater amount of matter is concentrated within very small volumes. Why matter should assume this form we do not know. At present we cannot give any reason why electrons should exist.

Ord. Int. Per.: If the mass of a body increases

with its velocity, why is it that the mass of a body has always been taken to be an invariant quantity?

Math. Phys.: The answer is obvious from the equation —

$$M = \frac{m}{\sqrt{(1 - v^2)}}$$

All values of v, that is, all velocities known until quite recent years, are very small in comparison with the velocity of light, and consequently $1 - v^2$ is very nearly unity, and so M very nearly equals m. The only bodies which permit us to test this equation are the particles shot out by radium. These bodies attain velocities very nearly equal to that of light. Their increase in mass can be measured, and is found to be what our formula predicts. You will notice that if $v = 1$, M is infinite. That is, a body moving with the speed of light has an infinite mass. This can only mean, once more, that the velocity of light is a critical velocity; velocities greater than that of light cannot occur in nature. Since the dimensions of a body in the direction of its motion contract in the same ratio as its mass

increases, we see that what we mean by a material body would become something quite different at the speed of light. It would become an infinitely heavy, infinitely thin sheet.

Phil.: You take your equations very seriously.

Math. Phys.: Well, of course, I do not know what would really happen to a body moving at the speed of light. What I have said is merely another way of saying that such a velocity, for matter, cannot occur in nature. The equations have been confirmed so far as we have tested them.

Ord. Int. Per.: So that the Newtonian dynamical equations have been experimentally disproved?

Math. Phys.: Yes. But the Newtonian equations were not revised in order to agree with the experiments on swiftly moving bodies. That came later. They were revised in order to fit our principle of relativity, namely, that the same laws of nature must be obtained by observers in uniform translatory motion with respect to one another. The Newtonian equations do not obey this criterion. Accordingly we modify them

until they do. We then find that experiment confirms the new equations. We assume that Newton's laws cannot be the true laws of nature since if they are true for one member of our group of observers they will not be true for the other members. We impose it as a principle that the true laws of nature must be the same for all these observers.

Ord. Int. Per.: But you have already said that different observers do *not* find the same values for different physical quantities, such as temperature, volume, mass, and so on.

Math. Phys.: I must explain myself further. Actual measured quantities may be different for different observers, but the *relations* between these quantities must be the same. Suppose, for example, that one observer measures, at a certain point, a quantity he calls magnetic force, and another quantity he calls electric force. He finds that there is a relation between them. He finds, we will suppose, that he can give precise mathematical expression to the way in which the time-rate of change of one of these quantities is connected with the space-rate of change of the other.

A second observer can also identify, at that point, a quantity he calls electric force and a quantity he calls magnetic force. Now he may not give these quantities the same numerical values as the first observer assigned to them. But what our principle asserts is that he will find the same *form* for the equation connecting the time-rate of change of one with the space-rate of change of the other as was found by the first observer. Unless he does this we say that one or both of the observers has not found the true form of the law connecting the changes in these quantities. Our principle serves as a criterion for the laws of nature. Newton's dynamical laws do not obey it. Maxwell's electro-dynamic laws, on the other hand, do obey it. It is the *form* of the laws of nature which is the invariant for which we seek, on our new theory. And in order to obtain these invariants we often have to make variable physical quantities which were formerly regarded as constant. To get an invariable form for the laws of dynamics, for example, we have to make mass variable.

Phil.: It is interesting to know that your prin-

ciple leads you to the successful formulation of laws of nature which obey it. But why should you adopt the principle? Why should you suppose that laws of nature must be expressible in a form which is invariant for uniform translatory motion of the system of reference?

Math. Phys.: The supposition springs from the impossibility of accepting the alternative. For supposing there is a privileged system of reference for which the laws of nature assume their simplest form, how are we to identify it? We have seen that the perfect Newtonian frame cannot be found. The æther, also, continually eludes us. Surely it is reasonable to suppose that the reason the privileged frame cannot be discovered is because it does not exist. In that case all systems of reference are equally valid, and the true laws of nature must have the same form for all these systems.

Phil.: But, in that case, why should you be content with a partial emancipation? Why demand laws which are invariant only for systems in uniform translatory motion with respect to one another? Why not insist that the true laws

of nature are those that are invariant for systems of reference having any kind of motion whatever?

Math. Phys.: You will see that that is precisely what we shall do. That is precisely the distinction between the generalised theory of relativity and the restricted principle I have been expounding to you. In fact, we claim that *any* system of reference is as good as any other. Our scales and clocks may be of any kind you please, they may be varying in any way you like, and yet they will constitute a frame of reference as good as any other. But before we come to that there are a number of other things to be discussed. I will endeavour to present the results of our discussion to you in a somewhat different way which will make easier the extension to the generalised theory. To make the formulæ more compact I shall call the four measurements between two events dx, dy, dz, dt. The interval between these events I shall call ds. Then, using ordinary units, the formula for the interval may be written $ds^2 = c^2dt^2 - dx^2 - dy^2 - dz^2$. Now what we have asserted is that two observers, in uniform translatory motion with respect to one

another, will adopt such space and time measurements as to obtain the same value for ds between the same pair of events. The value of the interval, ds, is invariant. As I have said, this formula is analogous to the formula expressing the distance between two points in a three-dimensional Euclidean space, or a four-dimensional or two-dimensional space. For three dimensions the space between two points would be given by $ds^2 = dx^2 + dy^2 + dz^2$. Now it is important to notice that from the formula for the distance between two points in space the whole geometry of that kind of space can be deduced. For instance, if the above formula is the formula for the distance between two points then we know that the space we are dealing with is a Euclidean space of three dimensions. If the formula were $ds^2 = dx^2 + dy^2$ we should be dealing with a plane surface, or with any surface which can be obtained from a plane surface (for instance, a cylinder) without distorting it. On the surface of a sphere, on the other hand, the distance between two points would not be expressed by a formula of the form $ds^2 = dx^2 + dy^2$. The geometry of a

spherical surface is intrinsically different from the geometry of a plane surface.

Ord. Int. Per.: But must the formula for the distance between two points on a plane surface always be of the form $ds^2 = dx^2 + dy^2$?

Math. Phys.: Certainly not. Instead of taking as your axes of reference two lines at right angles to one another and measuring dx along one line and dy along the other, you could have straight lines at any angle, or you could have a series of circles and radii, or you could have any curved lines for your axes of reference. The formulæ for ds would look very different in these different cases. But these differences would be due to the peculiar nature of the axes of reference you adopted. The intrinsic geometry of the surface would be the same. The point is, that on a plane surface axes of reference can always be found for which the formula for ds assumes the form $ds^2 = dx^2 + dy^2$, whilst on a spherical surface *no* axes of reference can be found for which ds^2 assumes that form.

Phil.: Then all the systems of reference that serve for a plane must have something in common.

Math. Phys.: Yes. All the systems of reference, or systems of *co-ordinates* as they are usually called in geometry, which serve for a given type of surface, have something in common. Gauss succeeded in giving this something in common precise mathematical expression, but the expression is rather complicated. Now let us return to our formula for the interval $ds^2 = c^2dt^2 - dx^2 - dy^2 - dz^2$. From what we have just said we see that this will determine the geometry of the four-dimensional world. We might have begun with this. We might have begun by saying that the space-time world has a geometry of a certain type, a type expressed by the above formula. We assert that there is a quantity, the *interval,* defined above, which is the same for all observers. We might then ask, what is the relation between the x, y, z, t of one observer and the x', y', z', t' of a second observer, supposing them to get the above formula for ds? This is a purely mathematical problem, and you would find the answer in the equations $x' = \beta(x - vt)$ and so on, that I have already given you. We should then find that the constant quantity v would be interpreted

as the relative velocity of the two observers. We still do not know what c is. Hitherto it is merely a constant in our formula for ds. But we should find that c plays the part of a critical velocity, that is to say, that in a world whose geometry is given by the above formula for ds, the combination of any number of velocities cannot exceed c. We have said nothing so far about the velocity of light. It is as a result of *experiment* that we identify the velocity of light with the critical velocity c. It is experiment which enables us to say that the value of c is, in fact, 300,000 kilometres per second. The velocity of light seems to have just the properties required of the c in our formula.

Phil.: So that if a new ray were discovered with a velocity greater than that of light you would no longer identify your critical velocity with the velocity of light?

Math. Phys.: No. That there is a critical velocity is a result of the formula we have adopted (provisionally) expressing the geometry of the four-fold order of events in nature. It is an indication that we are on the right track that an

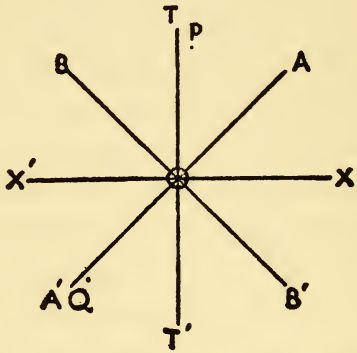
entity, the velocity of light, has been found which plays the rôle of the critical velocity.

Ord. Int. Per.: Is it at all likely that the critical velocity might, nevertheless, be different from the velocity of light?

Math. Phys.: A recent repetition of Fizeau's experiment on the velocity of light in moving water shows that it combines with the velocity of the water just as if it were the critical velocity. The experiment shows that it cannot differ from the critical velocity by more than 1 part in 500. As I have said, matter, referred to any one of our systems of reference, cannot have a velocity greater than the critical velocity c. In the expression for the interval $ds^2 = c^2dt^2 - dx^2 - dy^2 - dz^2$, if we divide both sides by dt^2 we get $\left(\frac{ds}{dt}\right)^2 = c^2 - v^2$ when v is the velocity of whatever is describing the interval. If v is less than c, ds^2 is positive, but if v is greater than c, ds^2 is negative, and the value of ds is imaginary. Intervals of the first kind are called time-like, and intervals of the second kind space-like. If v equals c, $ds = 0$, and this is, as you know, characteristic of light. The

points and moments for which ds is zero are given by the equation $0 = c^2dt^2 - dx^2 - dy^2 - dz^2$. This is the equation to a four-dimensional cone called the null-cone. The path of a ray of light lies on this cone. The advantage of this way of talking is that it enables us to give a graphical representation to the three-fold division of pairs of events that I have already mentioned, namely, if I take an event P, then there are events in the absolute future of P — all those that lie inside the null-cone radiating from P — events in the absolute past of P — all those which lie inside the null-cone when it is turned to point the other way — and all those which may be past, present, or future to P, depending on the observer. All these latter events lie outside the null-cones. As a very simple example of this graphical method let us consider a one-dimensional movement in space — a particle moving in a straight line. Represent this line by OX, and represent the time dimension by OT. Choose units so that the velocity of light is unity. The point O represents an event occurring at $x = o$, $t = o$. Suppose we are considering the movement of a

ray of light. Since it is moving with unit velocity the distance it describes is equal to the time it takes to do it, that is, $x = t$ all along its path. The lines OA and OB therefore represent its path,



since every point on them has gone as far in the direction OT as it has gone in the direction OX or OX'. These two lines correspond to the null-cone of the more general graphical representation. Now any event P which lies within AOB could be reached from O, because the velocity required is less than unity, that is, less than the velocity of light. Similarly, from an event Q in the section $A'OB'$ the event O could be reached with less than the velocity of light. The event Q

might be the event of a man falling off the top of a tower, O his crash on the pavement, and P his arrival at the hospital. For all observers Q precedes O and O precedes P. But events in the sections $B'OA$, $A'OB$ may be past, present, or future to O, depending on the observer. Thus, besides an absolute past and an absolute future there is an indeterminate region where the time ordering of events depends on the observer.

Phil.: Then if we cannot say of events within XOA, for example, that they are absolutely past, present, or future to the event O, it follows that the event O can have no influence on these events. For we could not say that there was a causal connection between O and some event in XOA if for some observer O could succeed that event.

Math. Phys.: That is so. The only events that O can effect lie within AOB. O could not affect anything in XOA since no influence could travel fast enough. If O is the present moment of my own life-history, for instance, then every event that has or might have influenced me lies within $A'OB'$. The past events that lie within $B'OX$ and $A'OX$ are past events which cannot

have influenced me. Similarly, every event that any present act of mine can conceivably influence lies within *AOB*. The events of *XOA* and *X'OB* belong to a future outside my possible control. Nothing that I can do will affect the events of *XOA* and *X'OB*.

Phil.: You do not think, then, that the events of *AOB* are rigidly determined in the sense that the events of *XOA* are rigidly determined?

Math. Phys.: I know nothing of what the ultimate justification of a philosophical determinism may be. All that I mean to assert is that, speaking in the language of everyday life, I can cause events within *AOB*. I can, for instance, cause an event at *P* — say the explosion of a shell. But nothing that happened on the sun within the last eight minutes can be affecting me at this moment, and, whatever technical powers I might have, I could affect no event that will occur on the sun within the next eight minutes. That is because light takes eight minutes to travel from the sun to the earth. Now, to sum up. The events of nature form a four-fold order. Between any pair of events is a relation, the *interval,* which

has the same value for all observers. We have assumed that this relation can be expressed in terms of measurements that we make with scales and clocks in the form $ds^2 = c^2dt^2 - dx^2 - dy^2 - dz^2$, where dt is the time between the two events marked by our clock, and $\sqrt{dx^2 + dy^2 + dz^2}$ is the distance between the two events as measured by our measuring rod. In order that the same measure of the interval for the same pair of events shall be reached by another observer, his time and space measurements must be related to ours by the formulæ I have given you. We have seen that these equations also express the fact that the two observers are in uniform translatory motion with respect to one another, and that the constant c plays the part of a critical velocity, the same for both observers. We have seen that there are excellent experimental reasons for identifying this velocity with the velocity of light. Further, we assert that all laws of nature must be the same for both observers. The laws of light, Maxwell's equations, are the same when referred to either system of measurement. The laws of dynamics, Newton's laws, have had to be revised

in order to obey the criterion. The revision leads to certain consequences which have been experimentally confirmed. The reality we have arrived at so far is a homogeneous four-dimensional continuum of semi-Euclidean properties. This continuum is separated into space and time, but differently by different observers. So much for the credit side of our achievement. I must now point out to you some imperfections of the theory. In the first place, the unconstrained motion of a particle in this space is a straight line, as on Newton's theory. The objection here is that we have to keep the Newtonian apparatus of attracting forces, in particular the force of gravitation, in order to account for the fact that the observed motions of bodies, such as the planets, not subject to impacts, are not straight lines. Secondly, the theory throws no light on the rigid and universal proportionality of a body's inertial mass to its gravitational mass. The third imperfection is one often mentioned by our philosophical friend; it is, indeed, a philosophic objection, but a very powerful one. Why should the laws of nature be invariant only for systems in uniform translatory

motion with respect to one another? In our next dialogue I shall begin to expound to you a theory, Einstein's generalised theory of relativity, which not only meets these objections, but does so by giving us a profounder and more comprehensive outlook on the world of physics than has ever been attained before.

FOURTH DIALOGUE

GENERAL RELATIVITY

Math. Phys.: The peculiar difficulty that everybody experiences in grasping the generalised theory of relativity is due to the fact that they start with certain prepossessions, certain assumptions, which the theory requires us to abandon. Some of these assumptions concern the relation between geometry and physics. I will therefore begin by discussing the status of geometry. You are familiar with the fact that for two thousand years the geometry of Euclid was regarded as the one and only possible geometry. The axioms which lie at the base of Euclidean geometry were considered to be necessary truths. From the logical consequences of these axioms, therefore, no escape was possible, and no alternatives were thinkable. We now know that the Euclidean axioms are not necessary truths. We know that postulates, incompatible with those of Euclid, lead

to perfectly self-consistent systems of geometry. Consider, for instance, Euclid's "parallel postulate" which asserts that, through a point external to a given straight line one and only one straight line can be drawn parallel to the given straight line. This postulate is not a necessity of thought; it is an assumption, and it need not be assumed.

Ord. Int. Per.: But how can that be? It is a statement which is either true or false.

Math. Phys.: What precisely do you mean? Do you mean that it is a statement of fact — that it is true or false of the behaviour of certain objective entities called straight lines?

Ord. Int. Per.: Well, it may be that there are no actual objective mathematical straight lines in nature.

Math. Phys.: It would not matter if there were. The proposition would then be a statement, justified by experience, concerning the behaviour of these natural entities. But that is very different from being a necessity of thought. We could imagine a different universe where this law of nature did not obtain.

Ord. Int. Per.: But does not the proposition follow from the very nature of a straight line, whether it exists or not?

Math. Phys.: In asking that question you are asking me whether the parallel postulate is not a necessary consequence of the definition of a straight line. You must therefore mean the straight line as defined by the axioms and postulates of Euclid *not* including the parallel postulate. You are asking, in fact, whether the parallel postulate can be deduced from the other axioms and postulates of Euclid. The answer is that it cannot. Attempts were made, for two thousand years, to deduce the parallel postulate from the other postulates of Euclid, but every one of these supposed proofs assumed a proposition equivalent to the parallel postulate itself.

Phil.: But that does not prove that such a proof is impossible, but only that it has not been obtained.

Math. Phys.: Precisely. The final proof that the parallel postulate could not be deduced from the others was given by the actual construction of

a non-Euclidean geometry. For this geometry accepted the other postulates of Euclid and denied the parallel postulate. If, therefore, the parallel postulate is implied in the other postulates of Euclid it follows that the geometry which denies the parallel postulate and accepts the others must develop contradictory propositions. But the geometry in question can be shown to be immune from all self-contradiction. It follows that the parallel postulate is not contained in the others, and that a perfectly self-consistent definition of the straight line can be given which does not obey it.

Ord. Int. Per.: But is this new straight line really a straight line?

Math. Phys.: Come now! You are asking me whether this straight line is a Euclidean straight line. I have just explained to you that it is not.

Phil.: I think he means to ask, Does it play the same rôle in the new geometry that Euclid's straight line plays in *his* geometry?

Math. Phys.: It does.

Phil.: Are we, then, at liberty to assume what geometrical postulates we like?

Math. Phys.: Yes. Provided, of course, that they are consistent with one another.

Phil.: So that we cannot say of any geometry that it is true or untrue?

Math. Phys.: No. A geometry is a body of propositions deduced from certain postulates. All we can say is that if we admit the postulates, then the propositions are logical consequences of them. But the primary postulates, provided they imply no contradictions, may be anything we choose. The fact is that a system of postulates is really a logical schema. It is an empty logical framework. We may, for instance, define a straight line in terms of points, or a point in terms of straight lines, but the terms "straight line" and "point," as they occur in the definitions, have no meaning. All we are concerned with is the *relations* between these terms. If you know of any entities which obey these relations, then you may take the propositions of the geometry constructed from these postulates as being propositions about these entities. A given

set of postulates may, for example, express the relations between certain algebraic equations just as well as the relations between the *geometrical* entities, points, straight lines, and so on. A geometry, in fact, has nothing essentially to do with space. It is a logical schema.

Ord. Int. Per.: The geometry of Euclid, then, considered as a body of postulates and the logical consequences of them, stands on the same footing as any other self-consistent geometry?

Math. Phys.: Precisely. I may point out, however, that Euclidean geometry has a certain advantage over all others. It is, intrinsically, the *simplest* geometry. It is the simplest geometry in the same sense that an algebraic equation of the first degree is intrinsically simpler than an algebraic equation of the second degree. This may be seen from the expression for the distance between two points in a Euclidean continuum. Consider a Euclidean continuum of two dimensions, that is to say, a plane. As I have said before, the formula for the distance ds between two adjacent points may always be expressed in the form $ds^2 = dx^2 + dy^2$. Similarly, the distance

between two adjacent points in a three-dimensional Euclidean continuum may be expressed in the form $ds^2 = dx^2 + dy^2 + dz^2$. Consider now an expression of the form $ds^2 = dx^2 + x^2dy^2$. This formula still refers to a plane, because we can transform it mathematically, as a matter of fact, into the old one, $ds^2 = dx^2 + dy^2$. But the formula $ds^2 = dx^2 + \cos^2 x dy^2$, cannot be transformed to the formula $ds^2 = dx^2 + dy^2$. There is an essential difference between these two cases. As a matter of fact the formula I have just given is the formula for ds on the surface of a sphere — a surface which is intrinsically different from a plane. A cylinder, on the other hand, is *not* intrinsically different from a plane, as we have already seen. Either of the formulæ $ds^2 = dx^2 + dy^2$ or $ds^2 = dx^2 + x^2dy^2$ would give the geometry of a cylindrical surface, since it has the same geometry as a plane surface. We cannot tell from mere inspection, therefore, whether the formula for the distance between two adjacent points refers to a Euclidean or a non-Euclidean continuum. The formula depends not only on the intrinsic geometry of the continuum, but also on the system

of co-ordinates employed. But, if the continuum is Euclidean, then co-ordinates can be found for which ds assumes the form $ds^2 = dx^2 + dy^2$ for two dimensions, with corresponding formulæ for three or more. The expression for ds, in any other geometry, cannot be reduced to this simple form.

Ord. Int. Per.: The geometry of a spherical surface, then, is a non-Euclidean geometry?

Math. Phys.: Yes, provided we confine our attention wholly to the surface. The shortest distance between two points on the surface of a sphere is an arc of the great circle — that is, a circle whose plane passes through the centre of the sphere — joining the two points. These arcs, therefore, would be selected, by a being confined to the surface, as his straight lines. Now all great circles cut one another. Accordingly, to this being, *no* straight line can be drawn parallel to a given straight line. Also, the sum of the interior angles of his triangles is not equal to two right angles. Also, if he drew a circle, that is, the locus of a point which moves in such a way as to remain at a fixed distance from a given point,

on the surface, the circumference of this circle would not be 2π times the radius. Of course, to use the geometry of the sphere is perfectly compatible with Euclidean geometry, because we should say that the being was not *really* talking about straight lines, but about arcs of circles. The *real* straight line joining two points on the surface of the sphere is, we should say, a Euclidean straight line which lies in a dimension of which the two-dimensional being has no cognisance. It is worth noting that a three-dimensional Euclidean continuum admits the presence of two-dimensional continua having an intrinsically different geometry. Now, seeing that Euclid's geometry is only one of an infinite number of possible geometries, the question arises whether the geometry of the four-dimensional continuum formed by the events of nature is Euclidean or not. We have seen that it is not, in any case, strictly Euclidean, since the space and time components of the interval occur with different signs. But what I am now asking is whether the geometry of this continuum is represented by the formula $ds^2 = dt^2 - dx^2 - dy^2 - dz^2$

where the co-ordinates dt, dx, dy, dz have constant coefficients, or whether ds should be given a more general form.

Phil.: What exactly do you mean when you ask whether the space-time continuum of nature has one geometry rather than another? You have just explained to us that a geometry is a hypothetico-deductive system of logical relations. It is a system of propositions deduced from arbitrary and consistent postulates. Also, your observations of external nature consist, you have told us, of observations of the readings of your measuring appliances. The theories you invent, therefore, to order and make coherent your measurements, are physical hypotheses concerned with the behaviour of physical entities. What is this space whose geometry you wish to determine? Is it not a purely mental construction, like your geometries themselves, and cannot you construct any space you like according to what geometry you choose? I fail to see, at present, what you mean by asking what is the geometry of the space-time continuum.

Math. Phys.: I must explain myself more fully.

The events of nature form a four-fold order. In order to arrange them coherently we have to adopt certain conventions of *measurement*. It was at one time thought that the Euclidean conventions were not conventions — that they were imposed upon us by the nature of things. We now know that this is not true. We are now free to inquire whether we can adopt more convenient conventions. I admit that we have freedom of choice; the four-fold order of events has no inherent geometry. But we find it necessary to attribute a geometry to it since we are concerned with measurements and our measurements must, of course, be coherent. Our criterion in choosing whatever geometry we shall choose is convenience, and this criterion has reference to the whole complex of phenomena. We cannot distinguish sharply between the geometry of events and the physical laws of events. The geometry and the physical laws are not independent of one another. Supposing we determined, for instance, that the three interior angles of a triangle formed by a fixed star and the two points at opposite ends of a diameter of the earth's orbit were not equal to

two right angles. To explain this result we can either alter our geometry and say that Euclid's geometry does not apply to the events of nature, or we can alter our physical laws and say that light is not propagated in straight lines. Which alternative we choose will be a matter of convenience. The assumption which explains the most facts in the simplest way is the one we shall adopt.

Phil.: What you call the geometry of the four-dimensional continuum, therefore, is merely one of the physical hypotheses you find it convenient to adopt in order to make coherent the events of nature?

Math. Phys.: Precisely.

Ord. Int. Per.: How do you propose to apply to the universe a geometry different from that of Euclid?

Math. Phys.: I will illustrate the general idea by an example which is of great intrinsic importance. We have assumed that, in the absence of a gravitational field, a body not subject to impacts moves with uniform velocity in a straight line. That is to say, we can find a system of co-

ordinates such that the body's motion, measured in this system, is a straight line described with uniform velocity. Now choose another system of co-ordinates, say a system rotating with respect to the first system. What is the motion of the body, as measured from this second system, supposing, of course, that the people using the rotating system (which may be a roundabout) imagine it to be at rest? It is no longer a straight line. And its departure from a straight line can be explained by forces acting on it, in this case fictitious forces introduced by adopting that system of co-ordinates. If we know the co-ordinates, and we can know them by analysing the expression for *ds* in terms of these co-ordinates, we can calculate these fictitious forces and so find the equations to the body's motion. The equations to the body's motion, therefore, involve quantities which appear in the expression for *ds*. It will make the argument clearer if I now write down the most general form for *ds* in four dimensions, a form which includes every kind of geometry, the Euclidean geometry and all the non-Euclidean geometries. For the nature of a geometry, you

remember, can be ascertained by analysing the formula for ds as it occurs in that geometry. This formula is —

$$\begin{aligned} ds^2 = g_{11}dx_1^2 + g_{22}dx_2^2 + g_{33}dx_3^2 + g_{44}dx_4^2 \\ + 2g_{12}dx_1dx_2 + 2g_{13}dx_1dx_3 + 2g_{14}dx_1dx_4 \\ + 2g_{23}dx_2dx_3 + 2g_{24}dx_2dx_4 + 2g_{34}dx_3dx_4. \end{aligned}$$

You observe that there are 10 g's in this formula. These g's are, in general, functions of the co-ordinates, that is, their values change when the values of x_1, x_2, x_3, x_4 change. Some of them may have constant values; some of them may be zero. All the different geometries can be obtained by giving appropriate values to the g's. We can obtain our old formula for the semi-Euclidean continuum, for example, by putting $g_{11} = g_{22} = g_{33} = -1$, and $g_{44} = 1$, and putting all the other g's equal to zero.

Phil.: What reason have you for saying that this expression is the most general form for ds?

Math. Phys.: All systems of co-ordinates with which we are familiar, Euclidean or non-Euclidean, are contained within this general form. We shall assume that any geometry we are likely to require can be expressed by this

formula. But there is not, perhaps, any *à priori* reason why a more complicated general form should not be necessary. You will find, however, that this formula is sufficiently flexible for our purposes. Now let us revert to our problem of the motion of a body acted on only by the fictitious forces introduced by the system of co-ordinates. The condition satisfied by its motion is as follows: the *interval* between two distant events in the history of the body is a maximum. This is a fact of observation. The body moves in such a way that the interval between two distant events is greater than it would be if the body moved in any other way. This means, for ordinary rectangular co-ordinates in the space-time continuum of the special theory of relativity, that the body describes a straight line with uniform velocity. We express the interval between two neighbouring events by ds. To get the interval between two distant events we add together all the adjacent little intervals ds. We may write this sum $\int ds$. The condition is that this sum is to be a maximum for the actual motion. For this to be so the co-ordinates of

the body at any time and place must obey certain equations. To find these equations is a purely mathematical problem. The actual path represented by the equations depends upon the values of the g's in the expression for ds. If the g's have the values mentioned above the equations represent a straight line described with uniform velocity. In other cases the equations will represent curved lines relative to the co-ordinates employed. The absolute locus of the moving body is, of course, the same, whatever system it be regarded from. We are now in a position to take a very important step. We have seen that the formula for ds varies not only with the kind of co-ordinates employed, but with the intrinsic nature of the space concerned. We may have any number of formulæ for ds which have the one characteristic in common that they refer to a Euclidean plane surface. But, as we have seen, we may have a formula for ds which does not refer to a plane at all, but to a spherical surface. And, just as for the plane, we may have any number of formulæ for ds whose one common characteristic is that they all refer to a spherical

surface. We need not confine ourselves to plane and spherical surfaces. We can have surfaces intrinsically different from both. Now it is evident that if ds refers to a plane surface the g's which occur in this expression cannot have just *any* values. There must be some relation between the g's which holds good for a plane surface and not for any other intrinsically different surface. Similarly, all the ds's which refer to a spherical surface will have something peculiar about their g's. Now the co-ordinates we have been considering hitherto, whether they be rotating, or executing a spiral motion, or what not, are, all of them, co-ordinates which belong to the semi-Euclidean continuum of the special theory of relativity. For we have always supposed that we could transform these co-ordinates to the standard co-ordinates for which all fictitious forces vanish. That is, we have assumed that the formula for ds can always be reduced to the form $ds^2 = dt^2 - dx^2 - dy^2 - dz^2$ by an appropriate choice of co-ordinates. For all systems of co-ordinates hitherto discussed, we have found that the equations of motion of a body, acted on by no

forces except fictitious forces, are given from the condition that $\int ds$ is a maximum. The step we propose to take is this. We shall assume that the motion of a body not subject to impacts is still given by the condition that $\int ds$ is a maximum, even when ds refers to a kind of space intrinsically different from the space of the special theory of relativity. That is, if the formula for ds is of such a kind that *no* mathematical transformation of co-ordinates can reduce it to the form $ds^2 = dt^2 - dx^2 - dy^2 - dz^2$ we shall still assume that the condition $\int ds$ is a maximum, gives the unconstrained motion of a body moving in that kind of space.

Ord. Int. Per.: You mean that the natural motion of a body, left to itself, may always be found from the condition that $\int ds$ is a maximum? This is your fundamental equation to such motion, whatever the system of co-ordinates employed, even when the co-ordinates are of a kind which can *not* be used in a Euclidean continuum?

Math. Phys.: Yes. We can *prove* that this condition gives the motion of a body in a Euclidean continuum whether the co-ordinates be

rotating or of any other kind that can be employed in such a continuum. We *assume* that it still gives the motion of a body in a non-Euclidean continuum. The point of this assumption is that we can now ask ourselves what kind of space-time continuum that must be for which the observed motions of bodies such as the planets are their natural motions.

Phil.: You are trying, then, to do without the notion of a force of gravitation?

Math. Phys.: Precisely. Instead of taking a Euclidean continuum *plus* a force of gravitation, I am trying to discover a non-Euclidean continuum which, in virtue of its geometrical properties — as expressed in the formula for ds — will explain the observed motions of bodies quite as well. The formula for ds for our space-time continuum ought to be of such a kind that the condition $\int ds$ is a maximum gives the observed motions of the planets without introducing any apparatus of forces at all. In other words, instead of explaining observed departures from straight line motion by postulating the existence of a gravitational field we shall try to explain

them by assuming certain appropriate but non-Euclidean metrical properties of the space-time continuum in which we order the events of nature.

Phil.: But then the geometry you assume must be different in different circumstances. In the neighbourhood of the sun, for instance, we will say that the space-time continuum has metrical properties such that the unconstrained motion of a body, say a planet, is an ellipse round the sun. But in the neighbourhood of a double or triple star a body, left to itself, would not describe an ellipse. The metrical properties of the continuum must therefore be different.

Math. Phys.: That is so. We shall find that there is no single formula for ds that can be extended over the whole universe.

Phil.: Then the geometry of your continuum is influenced by the presence of matter?

Math. Phys.: You can put it that way if you choose to regard matter as fundamental. But you will find that what we call matter may also be regarded as peculiarities of the space-time continuum. We can discuss this question more profitably later.

Phil.: Well, but since the *kind* of space-time continuum you assume varies according to the distribution of matter in the region cannot you, as it were, create *any* sort of space-time by suitably arranging your matter in the region?

Math. Phys.: No. The interesting fact is that one cannot. Let us revert to the general formula for *ds,* the formula containing ten *g*'s. Your question is, Can these *g*'s be any functions whatever of the co-ordinates? Can any and every kind of geometry occur in nature? The answer is in the negative. There are certain restrictions on the permissible values of the *g*'s. Let us see what the nature of these restrictions must be. They will be expressed by a set of equations between the *g*'s. Now suppose we know those equations and that we have ten *g*'s which obey them. We can, as we have seen, make mathematical transformations of these *g*'s, obtaining new systems of co-ordinates. We do not thereby alter the *kind* of space-time with which we are dealing. We have seen, for example, that we can have any number of systems of co-ordinates, all mathematically transformable into one an-

other, and all referring to a plane. Similarly we can have any number of systems of co-ordinates all referring to a spherical surface. But the g's of all the plane systems of co-ordinates will have something in common, that is, will obey certain equations, which will *not* be obeyed by the g's of the spherical systems of co-ordinates. If once we have found a set of ten g's, therefore, which obey the equations we are supposing ourselves to have found, all the sets of g's obtained by mathematical transformations from these ten must also obey the equations. This is so because the equations refer to the *kind* of space-time and hold good for all systems of co-ordinates appropriate to that kind of space-time. As the first condition for these equations to fulfil, therefore, we see that they must remain unaltered for all mathematical transformations of co-ordinates. This condition, by itself, does not, of course, enable us to take one step towards saying what kinds of space-time can occur in nature. But we have another condition that these equations must obey. We shall assume that the space-time of the special theory of relativity is of a kind that is possible in nature

—namely, at a great distance from all matter. We shall assume, that is, that at a great distance from matter the unconstrained motion of a body is a straight line described with uniform velocity. We shall call this kind of space-time *flat,* from its analogy with the Euclidean plane. Now the mathematical conditions that space-time shall be flat are known. If we have a formula for *ds,* and if *ds* refers to flat space-time, then the *g*'s which occur in that formula must satisfy certain equations, and these equations are known. These equations are obtained by equating a complicated expression involving the *g*'s, an expression called the Riemann-Christoffel tensor, to zero. The Riemann-Christoffel tensor is so called from the mathematicians, Riemann and Christoffel, who discussed it. The fact that the Riemann-Christoffel tensor must be zero for flat space-time gives us twenty conditions that the *g*'s must obey. Every set of *g*'s which obey these twenty conditions belong to co-ordinates which are appropriate for describing flat space-time. Now, obviously, these twenty conditions are too stringent for our purpose. They rule out every kind of

space-time except flat space-time. We want a set of conditions which are less stringent. They must allow flat space-time to exist, for we have agreed that flat space-time *can* exist in nature, but they must also allow certain varieties of *curved* space-time to exist. We therefore endeavour to modify the Riemann-Christoffel tensor so as to make it less stringent. This can be done. The modified tensor permits flat space-time, and it also admits a limited variety of other kinds of space-time. We get a set of conditions that the g's must obey if they are to describe the metrical properties of an empty region of nature, that is, a region not containing matter nor radiant energy. Accordingly we call these conditions the law of gravitation.

Phil.: There seems to me to be a very big jump here. Why do you suppose that your modified tensor has anything whatever to do with the intrinsic structure of the universe? The fact that it allows certain kinds of non-Euclidean geometries to exist is no proof that nature allows such kinds to exist. You have obtained it, I suppose, by juggling with the Riemann-Christoffel

tensor in some way. I have no doubt you have derived it in an ingenious manner. Probably you could obtain other modified tensors if you tried. But why am I to suppose that any of them represent conditions that obtain in nature?

Ord. Int. Per.: And why is a restriction on the possible geometries of the world called a law of gravitation?

Math. Phys.: Let me take your points in order. We can, as I have said, find the mathematical conditions that must be obeyed if space-time is to be flat. We can also find a set of conditions which will admit of the existence of a flat space-time and also of a group of space-times which are not flat. Now we are, if you like, engaged in *experimenting*. We are trying to see whether we can account for certain phenomena of nature in terms of different concepts from those that have hitherto been employed. Instead of introducing a gravitational force we are trying to account for the observed motions of bodies in terms of the geometry of the space-time continuum. To do this we must assume that the geometry is non-Euclidean, except at a great dis-

tance from matter. We have found a set of conditions which fulfil these requirements. We propose to adopt them and to determine their consequences. We cannot, I agree, *demonstrate* that the geometry of the space-time continuum is governed by these conditions. But, if the consequences of them agree with observation, we shall be justified in accepting them as we are justified in accepting any other scientific hypothesis. The second point is met by considering the purpose of our inquiry. We intend to explain phenomena, not in terms of gravitational forces, but in terms of geometry. A law governing fields of force, therefore, we shall interpret as a law governing types of geometry. So that in future when we speak of a gravitational field we are not referring to a region of the space-time continuum permeated by a force, but to a region of the space-time continuum which possesses certain geometrical properties. On the old way of speaking we should say that, due to the presence of the sun, there is a gravitational field, meaning that, in the neighbourhood of the sun, a material body would move in a certain way under the influence

of a pull from the sun. We shall now say that, in the neighbourhood of the sun, the interval between two events is expressed by a non-Euclidean formula, and in that kind of space-time a material body naturally moves in the way it is observed to move. That is, we say that the formula for ds is of a certain kind. Having found this formula we then use the condition — $\int ds$ is a maximum — to find the motion of the body. We dispense with the notion of force — at least so far as gravitation is concerned.

Phil.: Let us see exactly what assumptions this way of accounting for phenomena rests upon. You begin by saying that nature is a sum of events, and you wish to find certain relations between these events. Each event requires for its precise specification four numbers. You proceed to define a certain relation between two neighbouring events and you call this relation the *interval.* It is analogous to *distance* in space. You begin by expressing this relation by the formula $ds^2 = dx_4^2 - dx_1^2 - dx_2^2 - dx_3^2$. You find that if you do this you can identify x_4 with time and x_1, x_2, x_3 with the three mutually perpendicular

directions of space, as measured in the ordinary way. When this is done the results of the special theory of relativity can be deduced. Since the numbers x_1, x_2, x_3, x_4 in this formula correspond to space and time measurements you choose to interpret this formula as defining the geometry of the space-time continuum. It is astonishing that, from the geometry of the space-time continuum, so much can be deduced respecting the behaviour of natural phenomena as is deduced in the special theory of relativity. But the geometry defined by this formula is, it appears, a Euclidean geometry or, at least, a geometry very like Euclid's. Now you are familiar with non-Euclidean geometries. Corresponding to every one of these geometries is an appropriate formula for *ds,* and every one of these geometries is a particular case of a certain general formula. This formula is —

$$\begin{aligned} ds^2 = {} & g_{11}dx_1^2 + g_{22}dx_2^2 + g_{33}dx_3^2 + g_{44}dx_4^2 + 2g_{12}dx_1dx_2 \\ & + 2g_{13}dx_1dx_3 + 2g_{14}dx_1dx_4 + 2g_{23}dx_2dx_3 \\ & + 2g_{24}dx_2dx_4 + 2g_{34}dx_3dx_4. \end{aligned}$$

where the g's are quantities which may be constant or whose values may depend on the values

of x_1, x_2, x_3, x_4. Now if the g's have the following values: $g_{44} = 1$; $g_{11} = g_{22} = g_{33} = -1$; $g_{12} = g_{13} = g_{14} = g_{23} = g_{24} = g_{34} = 0$, we get the original formula $ds^2 = dx_4^2 - dx_1^2 - dx_2^2 - dx_3^2$, and we are dealing with the Euclidean continuum of the special theory of relativity. But it is not *necessary* that the g's should have these special values in order that ds may refer to a Euclidean continuum. Very different values of the g's may still refer to the same continuum. The mathematicians have, however, found that all the sets of g's which *do* refer to a Euclidean continuum obey certain conditions. You express these conditions mathematically by saying that a certain complicated expression, involving the g's, and called the Riemann-Christoffel tensor, vanishes. So that if we know the values of a set of ten g's, and substitute these values in the Riemann-Christoffel tensor, then, if the tensor turns out to have the value zero, we are dealing with a Euclidean continuum and, if the tensor does not turn out to be zero, then we are dealing with some kind of non-Euclidean continuum. If we are going to assume

that the space-time continuum is non-Euclidean, therefore, we cannot make it a condition that the g's make the Riemann-Christoffel tensor zero. But if we cannot assume that the Riemann-Christoffel tensor is to be zero, what conditions can we impose on the g's? What non-Euclidean geometry are you going to assume?

Math. Phys.: No *one* non-Euclidean geometry. That is, we assume no *one* definite non-Euclidean geometry which is to be applied to the whole universe.

Phil.: Precisely. You want, as it were, a very flexible general geometry so that at great distances from all matter and radiant energy it can be Euclidean and, in the neighbourhood of matter and radiant energy it can be non-Euclidean. But not one particular kind of non-Euclidean. The *kind* of non-Euclidean geometry is to vary according to the distribution of matter and energy you are considering. So that you intend to dower the space-time continuum with different geometries in different parts of it.

Math. Phys.: Exactly.

Phil.: At the same time, you do not allow the occurrence of just *any* kind of non-Euclidean geometry.

Math. Phys.: No.

Phil.: So that the g's cannot have just *any* values.

Math. Phys.: No.

Phil.: And you have, I understand, found a set of conditions such that, if the g's obey them, you can have Euclidean geometry and also a limited variety of non-Euclidean geometries.

Math. Phys.: Precisely. And that set of conditions is called Einstein's Law of Gravitation.

Phil.: Well, now, first of all, that this set of conditions corresponds to anything in nature is an *assumption.*

Math. Phys.: Yes. To be tested, like every other scientific assumption, by its consequences.

Phil.: Secondly, you use it in conjunction with another assumption, namely, that the invariable law governing the motion of a body not subject to impacts in this space-time continuum is that the interval between two distant events in the

history of that body is to be a maximum. Why have you chosen these assumptions?

Math. Phys.: Well, the conditions for the g's, the conditions which give Einstein's law of gravitation are, mathematically, the most simple set of conditions we can get, if we reject the vanishing of the Riemann-Christoffel tensor as too strict a condition, that is, if we decide that the space-time continuum is not to be Euclidean everywhere. We work out the consequences of these conditions first, therefore, before proceeding to more complicated conditions. As a matter of fact, we shall see later that there are deeper reasons for selecting these particular conditions, but we may at present regard their selection, as it was originally, as an assumption to be justified by its consequences. The second assumption, respecting the law governing the motion of a body, has also reasons that we will discuss later. But at present we may regard it as a natural extension to non-Euclidean continua of a law which is known, to a high degree of approximation, to hold good for a Euclidean continuum. Further, we shall take this law as giving us a clue to the metrical con-

ditions of the continuum in which the body is moving, and forego direct measurements made with scales and clocks. But the observed motion of a freely moving body, relative to some system of co-ordinates is not, by itself, sufficient to determine the metric properties of the continuum. So we must introduce a further condition. We saw, in discussing the special theory of relativity, that the interval between two events in the history of a light ray is zero, that is, $ds = 0$ for light. We shall assume this condition to be generally true. In order, therefore, in any particular case, to obtain the values of the g's from directly observable phenomena, we shall assume that, for a moving body not subject to impacts $\int ds$ is a maximum and, for a ray of light, $ds = 0$.

Ord. Int. Per.: You have said that the assumptions made by the new theory of gravitation are to be justified by their consequences. But does not Newton's theory of gravitation give an exact account of observed phenomena and must not the new theory, therefore, lead to the same results as Newton's?

Math. Phys.: Newton's theory gives so accu-

rate an account of phenomena that it is certain that the new theory, if it is to be accepted, must lead to results which are, at most, very little different from the results calculated on Newton's theory. It is strongly in favour of the new theory, therefore, that in practically every case it leads to results which cannot be observationally distinguished from those of Newton's theory. There are three cases, however, where Einstein's theory leads to results which do not agree with the Newtonian theory. In each of these three cases observations agree with Einstein's theory and not with Newton's. The first case in which Einstein's theory shows its superiority is in connection with the motion of perihelion of Mercury's orbit. According to Newton's theory, a planet, if not disturbed by the influence of other planets, should move round the sun in an ellipse. On Einstein's theory such an isolated planet would not move accurately in an ellipse. The actual orbit would be that of a rotating ellipse, so that the perihelion, that is, the point of the ellipse nearest to the sun, would itself move in the direction of the planet's motion. This

motion of perihelion is very slow. In the case of a planet like the earth this advance of perihelion would be very difficult to observe, for the orbit is so nearly circular that the point of perihelion is very ill defined. The most markedly elliptical orbit in the solar system is the orbit of Mercury, and here an advance of perihelion had long been observed. Now, even according to the Newtonian law an advance of perihelion of Mercury's orbit was to be expected, due to the influence of the other planets of the solar system. This expected advance was 532 seconds of arc per century. The observed advance of perihelion is 574 seconds of arc per century. Thus there is an advance of 42 seconds of arc per century unaccounted for by calculations based on Newton's law of gravitation. Now the advance of perihelion for Mercury due to the fact that its orbit is not exactly an ellipse, as calculated by Einstein's theory, is 43 seconds of arc per century. As you see, the concordance is very striking, and strongly inclines one to admit the validity of Einstein's assumptions. The second case is concerned with the deflection light experiences in

passing through a gravitational field. It is a consequence of the theory of relativity that light is propagated strictly in straight lines only in a region remote from all matter. A ray of light from a distant star, in passing close by the sun, should undergo a certain deflection. Hence the star should appear to be displaced by a certain amount. The amount of this deflection can be calculated, but, obviously, it cannot usually be observed because when the sun is in the heavens, stars, or certainly those in its neighbourhood, cannot be observed. It is only during an eclipse that stars in the line of sight of the sun become visible. As a result of two eclipse expeditions the deflection predicted by Einstein's theory has actually been observed. It may be mentioned that Newton's theory also will give a certain deflection. This is so since light is a form of radiant energy, and, as we have seen, mass and energy are convertible terms. Hence mass may be attributed to light and it may be assumed to be subject to the attractive force of Newton's theory of gravitation. But the deflection so calculated is only one-half of that predicted by Ein-

stein's theory. The actual observations made at the two eclipses leave no doubt that the deflection has the value predicted on Einstein's theory and not that predicted on Newton's theory. The third case concerns the time of vibration of an atom on the sun compared with a similar atom on the earth. It can be shown that an atom on the sun should vibrate more slowly than an atom on the earth, owing to the greater intensity of the sun's gravitational field. The lines in the solar spectrum, therefore, compared with the lines emitted by the same elements on earth, should appear displaced towards the red end of the spectrum. This result, also, has been confirmed by observation.

FIFTH DIALOGUE

SPACE-TIME AND MATTER

Math. Phys.: I now wish to describe the position occupied by matter in the new theory. We have seen that the empty space-time continuum in the region of matter enjoys geometrical properties which differ according to the distribution of matter in the region. It is therefore natural to regard matter as an extraneous substance existing in space-time and somehow modifying its geometrical properties in a certain way. We suppose, in accordance with our ordinary habits of thought, that space-time is one thing and matter is another. It is true that we have shown that there is an action of matter on space-time which was not suspected by the older theories, but, although the distinction between them has become a little confused, we still suppose that matter is something essentially different from space-time. I wish now to suggest to you that we need not

continue to think in this way. We begin by considering the universe as consisting of point-events. Between every pair of events there is a fundamental relation we have called the interval. By giving this relation mathematical expression we have obtained the quantities we have denoted by g_{11}, g_{22}, etc. From these quantities we can, by purely mathematical analysis, obtain a number of others. Now let us consider what the concepts of matter, energy, momentum, stress, mean to us in physical science. They are entities which have been observed to obey certain relations. Mass or energy, for instance, obey the relation expressed by saying that it is *conserved.* This relation can be given mathematical expression. Now, amongst the mathematical expressions derived from the g's is one which obeys precisely this relation. It obeys the relation of conservation. In the one case we use certain symbols standing for such concepts as *density, velocity, energy,* and so on. We form equations in which these symbols appear, and these equations summarise the observed behaviour of the entities we have called density, energy, and so on. These equations are

said to express laws of nature. In the other case we have a different set of symbols referring apparently to a quite different class of concepts namely, concepts derived from the fundamental interval relation we have postulated. It is found that these symbols obey exactly the same relations as the symbols representing density and the like. May we not, then, identify these symbols? All the mathematical expressions derived from the g's refer to the space-time continuum. An equation satisfied by one of these expressions represents a certain property of space-time — it may mean, for instance, interpreting the expression geometrically, that where that equation holds good, the space-time continuum has a certain curvature. We find that exactly the same equation indicates to us, it may be, that matter is present. Cannot we say, therefore, that space-time being curved in this way *means* that matter is present, or that matter being present *means* that space-time is curved in this way? Consider, for instance, a piece of matter. It has certain properties which interest us, as physicists. It has, for instance, *density, velocity, internal*

stresses, and so on. These physical quantities obey certain mathematical relations. Now we can form from the g's a mathematical expression which has as many components as there are physical quantities belonging to the piece of matter. And these components obey exactly the same mathematical relations as do the physical quantities. May we not say, therefore, that these components express certain features of the space-time continuum which manifest to us as density, stress, and the like?

Phil.: I am not quite clear as to what you mean. The equations obeyed by physical quantities such as density and so on, summarise our experience of matter. They are the result of observation. Are your mathematical expressions, derived from the g's, *observed* to obey these same equations, for, if so, what is it you observe?

Math. Phys.: No. Obviously not. I am suggesting that we observe the mathematical components when we observe the physical quantities. If the mathematical components had any different existence, susceptible to observation, they obviously would not be the physical quantities. No,

the mathematical components obey their equations by mathematical necessity.

Phil.: Then, if they are the physical quantities, the physical quantities must obey the observed laws of nature by mathematical necessity.

Math. Phys.: Precisely. The observed laws of material behaviour are a necessary consequence of the existence of the interval relation.

Ord. Int. Per.: Is the existence of matter a necessity also?

Math. Phys.: Ah! now you raise a question which is concerned with the very nature of our minds. All I can say is that, granted a universe of which we can postulate the interval relation, then there are certain aspects of that universe which will have the quality of *permanence.* Our mind, it appears, pays particular attention to these aspects. I don't know why. It singles them out, and dowers them with substantiality, giving them names such as matter, energy, momentum.

Ord. Int. Per.: But could there be any mind for which matter did not exist?

Math. Phys.: You mean, can I grant the pos-

sible existence of a mind not specially interested in those aspects of the universe which obey the condition of permanence? Certainly I can. I have not the faintest idea what it would be like, but it is no more difficult to admit as a possibility than to admit the existence of minds, like our own, which are interested in the permanent.

Ord. Int. Per.: Then matter does not *really* exist?

Math. Phys.: Of course it does. It is indubitably an element of the universe.

Phil.: I think our friend means to say that matter does not exist apart from our minds. It seems to me, that, taking your own theory, you must admit that he is partly right. Without going into metaphysics at all it seems to me that what *is* matter on your theory, these mathematical components, *need* not in the least have appeared to us as matter.

Math. Phys.: Certainly they must have done so, for matter is only something which obeys certain relations, namely, the relations obeyed by the mathematical components I have mentioned. But I have already said that I can grant the exist-

ence of minds for which these relations are of no special interest.

Ord. Int. Per.: Then for those minds matter would not exist?

Math. Phys.: No.

Ord. Int. Per: But, you say, matter would nevertheless exist?

Math. Phys.: It would exist for such minds in the same sense that certain aspects of the universe exist which we do not perceive. From the fundamental interval relation we can obtain mathematical expressions which express properties of the space-time continuum, but properties of which we seem to have no direct perception. We can say, nevertheless, that these properties exist.

Phil.: I am not sure that you can.

Math. Phys.: Well, I can say that they exist in the sense that they are logical consequences of the existence of the interval relation. I do not wish to discuss this point more fully. I am not at all sure, myself, that consequences of certain axioms exist in the same sense that undiscovered minor planets exist, for example. But it would take me too far from my present purpose

to go into the philosophical discussion. What I am certain of is that two factors are concerned in the existence of the material universe we know, the fundamental interval relation and the nature of the human mind. Into the implications of that remark I do not care to enter.

Ord. Int. Per.: You have said that the laws of nature or, at any rate, the laws of mechanics, are mathematically necessary. Do you mean that the world could not be other than it is?

Math. Phys.: One could almost say that the laws of mechanics are a consequence of the mind's attempt to discover something permanent in the universe. The law of conservation, which expresses permanence, does not hold except in the space-time system we have, in fact, adopted, the space-time system I explained to you in discussing the special theory of relativity. Is it too much to say that the mind's recognition of space and time, and of the sort of space and time it does recognise, is due to its search for something permanent in the world? When the mind has achieved this feat, when permanence can be predicated of some aspect of the universe, then it

appears that the laws of mechanics are a necessary consequence of this achievement. It appears also, on this more advanced view, that Einstein's law of gravitation for empty space is not a restriction on the fundamental structure of the world. It receives a new interpretation. The law of gravitation for empty space merely *means* that matter is not present. The law of gravitation for empty space may be expressed by saying that certain mathematical expressions, derived from the interval relation, are zero. If this is true then we are dealing with a vacuum. But if it is not true we are not dealing with a vacuum, but matter is present.

Phil.: Then Einstein's law of gravitation, on this new view, must not be interpreted as saying that only certain kinds of geometry can be applied to the space-time continuum. *Any* kind of geometry can be applied?

Math. Phys.: Yes. But when certain kinds of geometry become applicable, we say it is an indication that matter is present. The geometry of a region continuously filled with matter might be anything — depending, of course, on the values

of the physical quantities defining the matter. As a matter of fact some kinds of geometries, even so, are not applicable to nature as we know it. We could, for instance, find that a certain kind of geometry could only correspond to matter having a density much greater than any which has been observed.

Ord. Int. Per.: By identifying matter with a local peculiarity of the space-time continuum do you account for all the known properties of matter?

Math. Phys.: No, it must be confessed that our identification extends so far not to the whole complex of properties we call matter, but only to its mechanical properties. Our theory throws no light, for instance, on the atomic constitution of matter.

Phil.: Nor, I suppose, on the phenomena of living matter, to say nothing of the fact that consciousness seems to be associated with certain material structures, to wit, our brains?

Math. Phys.: No, our theory throws no light on these properties of matter.

Phil.: So that your mathematical expression,

derived from the interval relation, does not, after all, play the part of matter in a completely convincing fashion. You are talking merely of matter as it concerns the physicist — a mechanical abstraction. Matter, as it concerns the biologist and psychologist, does not enter your scheme.

Math. Phys.: Not yet. And, as I have said, matter, even as it presents itself to the physicist, is not completely derivable from the interval relation. There remains the enigma of its discontinuous structure.

Phil.: I am not quite clear as to the logic of your exposition, even so far as you have geometrised matter. You have certainly been talking about the space-time continuum as if it were an objective entity. This entity possesses, it appears, different geometrical properties in different regions. In some regions, where it has a particular kind of structure, our organs of perception interpret that fact by saying that matter is present. In other regions, possessing a different structure, we say that there is a vacuum. But this space-time continuum, that you thus make the fundamental substratum of the material

universe, was introduced by you, not as an objective entity, but as a device useful in ordering and making coherent the data of experience. It is *convenient* to assume this method of representing experience, as we might assume a Euclidean plane in which to represent, graphically, the relations between the pressure and volume of a gas. But if your space-time continuum is no more than that, what precisely do you mean by saying that matter *is* a peculiarity of this continuum?

Math. Phys.: When you speak of space-time you are probably giving the term a richer content than I give it. The "space-time" of the physicist may be like the "matter" of the physicist. Just as the "matter" of the physicist is not an adequate summary of all we mean by matter — in our capacities as biologists and psychologists — so the space-time of the physicist may not be completely adequate to the space-time of experience. But I am talking about a physical theory, a theory belonging to the science of physics. I am not attempting to construct a world philosophy. I am limiting myself to certain aims, and I am not concerned with space and time except in

so far as these words *mean* the kind of relations I am interested in. As a human being I have an apparently unconquerable psychological predisposition to mean more by space and time than the mere sets of measurable relations these terms mean to me in physics. But let us look again at our fundamental assumptions. We start with a world of *point-events.* We do not define a point-event. Every theory must begin with undefinables. The word point-event calls up to our minds the conception of a point at an instant, that is, it makes appeal to the concepts space and time. But, to begin with, we must not allow the word to do that. The notions of space and time are not to be employed at present. Whatever point-events may be we say that the total aggregate of all point-events is the world, and, further, we say that the world is four-dimensional. This means that every point-event has, corresponding to it, a set of four numbers. We shall further assert that between every pair of point-events is a certain relation called the *interval.* We cannot say what is the real nature of this relation any more than we can say what is the real nature of a point-

event. But just as a point-event has a group of four numbers associated with it, so the interval relation has a numerical aspect. We can write down a formula expressing this numerical aspect of the interval relation. Now, having done this, we find that we *can* give this formula a *geometrical* interpretation, that is, we use the notion of a spatio-temporal displacement to represent this relation. The reason is psychological, and physics is not concerned with it. It is not, so far as we can see, necessary that we should represent this aspect of the interval relation to ourselves in this way. It is a tendency of the mind, but, although more fundamental and persistent, it may be a tendency like the tendency to refer certain phenomena to "forces," from which the mind can, at least partially, emancipate itself. However that may be, it is at this point that space and time enter physics. That the ultimate indefinables of the world have relations which bestow a *structure* on the world is all that we have assumed. That our fundamental relation, the interval relation, can be represented geometrically as a spatio-temporal relation, is a further assumption.

Phil.: Might it not be that the incompleteness, which you have admitted, of your derivation of matter from this fundamental relation is due to your assumption that it can be represented, exhaustively, as a spatio-temporal relation?

Math. Phys.: That may well be. It is possible that space and time are conceptions which are, as it were, too crude for physics.

Ord. Int. Per.: Perhaps you will have to find out more about the real nature of the interval relation of which you say you know nothing but its numerical aspect.

Math. Phys.: That, I am afraid, would lead us into philosophy, and outside what has hitherto been meant by physics.

Ord. Int. Per.: But why should not physics grow in such a way that it *does* become something different? Who can say within what limits physics *must* be confined? Why should not physics find, for instance, that as it continues to study matter, it is also really studying mind?

Phil.: It certainly seems possible to me. There are already philosophers who think that

matter and mind are different arrangements of the same fundamental "neutral stuff."

Math. Phys.: Then it is only one class of these arrangements with which physics is concerned, the class that is matter.

Phil.: Why should you suppose that the two classes of arrangements are entirely independent of one another?

Math. Phys.: We have talked enough philosophy, I think. Let us get back to what does certainly belong to the science of physics. You will notice that, in postulating the interval relation between point-events, I have not said anything about its measurability with scales and clocks. We have started by postulating a continuum of four dimensions where pairs of points are connected by a relation I have called the interval. Our object is to work out the mathematical properties of this continuum, and to see whether the continuum, so developed, throws any light on the real world. In order that we may do so we must, at some point, identify the quantities occurring in our mathematical analysis with quantities which occur in experience. In the exposi-

tion I have just given you we can delay making the identification until, in the course of our analysis, we have arrived at the quantities which we have identified with energy, stress, and so forth. Until we make this identification we simply go on obtaining one mathematical expression after another, none of which have any meaning except as being logical consequences of our postulates. We have delayed making this identification until we have arrived at an expression which shall serve as matter, and by doing so we have obtained light on the rôle played by space and time in the theory. If we had identified the interval, directly we had obtained it, with a quantity measured by scales and clocks, we should already have introduced the notions of space and time and also of matter, since scales and clocks are material appliances. The delay, therefore, is logically justifiable. The first point where we make contact with the real world is in identifying a certain expression as matter. Our grounds for making this identification are that the expression we reach obeys the law of conservation. Having done this we see at once that Einstein's law of gravitation

for empty space is merely an expression of the fact that empty space is empty, that is, that there is a complete absence of anything we could perceive as matter. In our previous discussions we have obtained a quantity, the interval, which is the result of measurements made with matter. It remains to be seen whether the interval we have now postulated is the same as the interval we obtained by measurement, that is to say, we have to see whether matter, as defined by the mathematical expression we have obtained from our postulates, can bring us to the interval regarded as the result of measurements made with this matter. But before entering upon this we must consider the question as to whether the universe is infinite or finite.

Phil.: Before you go on to consider what sounds a very interesting question I should like to know whether your mathematical expression, which plays the part of matter, is unique. The only reason you have given for supposing it to be matter is that it obeys the law of conservation. Can you derive any other mathematical expressions which also obey this law?

Math. Phys.: Yes, we can derive other more complicated mathematical expressions which also obey the law of conservation. But since they obey the same law there is no advantage in employing them. If they express the same set of relations then, for our purpose, they are the same thing. It is true that these more complicated expressions might, even so, give a different law of gravitation, that is, the solutions which make them equal to zero might give different geometrical properties for empty space. But it can be shown that the solutions which satisfy Einstein's law also satisfy these more complicated laws. Other solutions, which satisfy these more complicated expressions but which do not satisfy Einstein's law, do not seem to occur in nature. And any differences which might occur are too small to be observed. But I am about to discuss with you a modification of Einstein's law which Einstein himself has made, and which leads to the conclusion that our universe is spatially finite although it is unbounded.

Ord. Int. Per.: What is the difference between being finite and being unbounded?

Math. Phys.: I can illustrate the idea by asking you to imagine a spherical surface. A being confined to this surface can move in any direction on the surface without encountering any boundary. Nevertheless the area of the surface — the "space" of the being — is finite. The expression for the distance *ds* between two points belonging to the two-dimensional space which is the surface of a sphere involves two variables, and a constant which is the radius of the sphere. We may extend this for a three-dimensional spherical surface involving three variables and a constant which we can interpret as a radius of curvature. The complete expression for the *interval* is, of course, four-dimensional, involving, besides the three variables pertaining to the spherical space, a variable pertaining to time. Einstein suggests that our space, considered on the grand scale, is of this kind. The presence of matter means that space is not strictly homogeneous. It is only when we consider space as a whole that we can describe it by this spherical formula. According to this theory the volume of space is finite, but there is no boundary. Every point stands in the

same relation to the rest of space as every other point.

Ord. Int. Per.: What is the purpose of this theory? What reason is there for supposing that space is finite?

Math. Phys.: The reason is connected with a problem I discussed with you in our first dialogue. I there made it an objection to Newton's theory that the property of inertia was regarded as something absolute. Newton supposed that a solitary particle of matter, supposing no other matter to exist in the universe, would possess the property of inertia and further, its inertia would remain unaltered supposing the rest of the material universe were to come into existence. This belief leads to consequences which offend one of our cardinal principles, namely, that the relation of cause and effect should only be postulated to exist between observable facts. Now our theory of gravitation, as we have hitherto developed it, supposes that, at a great distance from matter, space-time is flat, that is, that it has the characteristics attributed to it in the special theory of relativity. We have assumed that space-time is

infinite and that the limiting conditions for space-time at infinity are that space-time is flat. Now the inertia of a solitary particle in such a space-time would have a value very little different from its value in the presence of other matter. Thus we have assumed that although the inertia of a particle can be *influenced* by the presence of other matter it is not *conditioned* by it. Thus, if we demand that inertia shall be relative, this assumption is unsatisfactory. We cannot, in accordance with our principle, accept an inertia relative to space, but only an inertia which is relative to other matter. Accordingly, as a mass is removed farther and farther from other masses, its inertia must ultimately fall to zero. Now when the conditions for this are formulated mathematically they are found to conflict with a fact of observation, namely, that the average velocities of the stars are very small compared with the velocity of light. It would seem, therefore, that we must return to our first assumption and say that, at an infinite distance from matter, space-time is flat. But it occurred to Einstein that the problem can be solved in another way,

without giving up the relativity of inertia. This way is to assume that space-time is finite.

Phil: That space-time is finite, therefore, is an assumption made in order to settle an epistemological difficulty. To one who is quite willing to admit that matter at an infinite distance from all other matter may still possess inertia the assumption is unnecessary. Are there no other grounds than this for believing space-time to be finite?

Math. Phys.: Well, the assumption of a finite radius of curvature for space seems to throw some light on the fact that matter is something which has a limited extension in space, but an unlimited duration in time. Consider, for instance, an electron. It has a certain definite extension in space — it has a finite radius. But it has no definite extension in time. Its extension in time persists indefinitely. I suggest that this behaviour is a reflection of the electron's adjustment to the particular kind of space-time continuum in which it exists. Its spatial dimensions express its adjustment to the spatial peculiarities of the continuum. We may regard the radius of an electron

as determined, in some way, by some condition of the space in which it exists. If we assume that this condition may be expressed as a *curvature* of the space, then we may say that the radius of an electron is a certain fraction of the radius of curvature of space. If this can be admitted then, since the radius of the electron is finite, the radius of curvature of space must be finite. Now Einstein's finite space-time continuum is *not* curved in the time dimension. Hence there is no finite duration of time to which the electron has to adjust itself. Consequently the electron's extension in time, unlike its extension in space, is indefinite.

Phil.: In this reasoning, which seems to me a little fantastic, you assume that, wherever in space the electron may be, its radius is the same fraction of the radius of curvature of space?

Math. Phys.: Yes, and by so doing we see that the isotropy we have attributed to our spherical space is a necessary consequence of the operation of measurement. Even if the radius of curvature were different in different parts of space we should

never discover this difference for our measuring rod would, by hypothesis, so adjust itself to the different conditions as always to be the same fraction of the radius of curvature, and hence give the same numerical value for that radius. In an anisotropic space a material structure would need to have the same anisotropy in order to be in equilibrium. Since this same anisotropy would affect all material measures it could not be discerned by measurement.

Phil.: What sense, then, is there in supposing it could exist?

Math. Phys.: In order to show that the symmetry and homogeneity of space need not be postulated of the fundamental structure of the world, but is a consequence of the operation of measurement. The homogeneity of space, expressed by Einstein's law of gravitation in its new form, is not therefore to be interpreted as a remarkable limitation on the general relation-structure with which we began.

Phil.: You say that the space-time continuum, on this new theory, is uncurved in the time direction, but curved in the spatial directions. There

is therefore a distinct difference between space and time on this theory. You can no longer say that the distinction we make between space and time corresponds to no objective reality.

Math. Phys.: Yes, it is true that for phenomena on a cosmical scale there is a distinction between space and time. Also, we have assumed, on this theory, the existence of a frame of reference for which the average velocities of all material bodies are small. So that, to some extent, absolute velocity is restored. But this only applies to the universe considered as a whole. For local phenomena our earlier views continue to hold. On Einstein's view matter is essential to the existence of space and time at all. Without matter the universe would shrink to a point. According to Einstein both the volume of space and the law of gravitation are conditioned by the total amount of matter in the world. A lower limit for the total amount of matter can be given on this theory. The mass of all the matter in the universe must be at least equal to a trillion times the mass of the sun. Also, the actual size of space can be given approximately. A ray of light would

go all round the universe in about 1000 million years.

Phil.: Einstein's view, that matter is essential to the existence of space and time, is surely not a consequence of the theory you formerly outlined to us, that matter is derived from the primary interval relation?

Math. Phys.: No, I do not think it is. The part of the theory I am now describing to you is still very speculative. It may even be, as Dr. Whitehead argues, that the space and time adopted in this theory of general relativity is not the space and time of perception. But I think that if you keep fast hold of the fact that the space and time I am talking about is introduced as a method of ordering experience you will not be led into confusion. Its relation to what I might call the space and time of psychology is a matter for investigation, but it is not an investigation that I propose to go into now. The way in which space depends upon matter in Einstein's finite universe may be seen from the simple equation —

$$M = \tfrac{1}{2}\pi R.$$

Here M is the total mass of matter in the universe, and R may be called the radius of space. We see that if M decreases R also decreases. We could imagine M decreasing by pairs of positive and negative electrons coalescing. M is here the gravitational mass expressed as the radius of a sphere. The gravitational mass of an electron can be expressed as a length — as the radius of a sphere — but this is not the same as the electrical radius of an electron, which is what we call the actual radius. So that if we talk of R decreasing it is not clear what fixed standard of length is implied. R is itself the natural unit of length. We can interpret the above equation by taking R as unity, and then —

$$M = \tfrac{1}{2}\pi.$$

M is now a constant, so that if the total number of particles in the universe decreases, the mass of the remaining particles must increase in the same proportion. It is as if each particle tried to monopolise all space but was prevented from doing so by pressure exerted by the other particles.

Phil.: All this sounds very speculative.

Math. Phys.: Yes, they are no more than suggestions which illustrate the kind of problems that arise in the theory of relativity. But some authorities find a sort of justification for these views in the fact that the ratio of the radius of an electron to its gravitational mass is a very large number, no less than 3.10^{42}. The suggestion is that this number may be connected with the actual number of particles in the universe. Possibly the square of this number, for example, may be equal to the total number of positive and negative electrons that exist. It is a speculation congenial to some minds that the laws of nature may be determined by the actual number of particles that exist.

Phil.: Is not this very like Pythagorean mysticism?

Math. Phys.: Well, although the Pythagoreans found significance in many analogies we now consider ridiculous, we need not assume that their general outlook has no grains of truth whatever in it. However, whatever may be said about these modern speculations it is certain that if they are ever justified it will be by scientific

criteria and methods. There is no harm in a scientific man trying to get a first glimpse of truth by letting his imagination roam freely. He will only be in a position to enunciate it as a truth when he can adduce the kind of evidence for it that science demands.

Ord. Int. Per.: It is very difficult for me to imagine that the universe is spatially finite. Why should it not be also temporarily finite? If by going on and on, as a ray of light does, one comes round again to one's starting point, why should not progress in time be of the same kind, so that one would come round to the same events and everything that had happened would happen all over again?

Phil.: The theory of eternal recurrence that Nietzsche tried to popularise.

Math. Phys.: I do not think much is to be gained by trying to *picture* a spherical space. You must remember that the space-time continuum, on our theory, arises as a particular way of interpreting the interval relation. The properties of this space-time, therefore, tell us something about the connections of intervals. To say that

space-time has such and such relations, therefore, is to say that the network of intervals is connected in such and such a manner.

Ord. Int. Per.: Then are you talking about real space and time at all?

Math. Phys.: Yes, in the sense that we certainly suppose ourselves to be making true remarks about the behaviour of measurements commonly called spatial and temporal. Whether you explore the metrical properties of space-time by scales and clocks, or whether you explore them by the paths of freely moving bodies and light rays, we believe that our formulæ express what you would observe to be the behaviour of these measuring appliances. But I must warn you that there is an alternative theory, propounded by Dr. Whitehead, which denies that space and time have these properties, and yet claims to give the same results as our theory when it has been tested. My purpose, however, is to explain to you Einstein's theory — what I might call orthodox relativity theory. If you like to keep the notion of a Euclidean space-time I think you can do so if you imagine some kind of æther which affects our

measuring appliances in the way described by our formulæ. But you will find this æther very different from the old one, and, indeed, you may discover that there is very little gained by postulating it — except, possibly, as satisfying certain philosophical requirements. As to whether the time dimension may also be curved, there is a formula, that of De Sitter, which also gives a finite universe, but one whose shape is that of a four-dimensional spherical surface, so that the time dimension is curved as well as the spatial dimensions. Nevertheless, eternal recurrence does not occur. We may call this a *spherical* universe in contra-distinction to Einstein's, which we may call a *cylindrical* universe. Eternal recurrence does not occur because this universe is open in the time dimension. This universe enjoys very extraordinary properties. For instance, the *time* between two events is longer the farther they are away from us. A vibrating atom, for example, would seem to us to vibrate more slowly the farther it was away from us. There is a maximum distance, the distance of the "world horizon," as it is called, where it would appear

not to be vibrating at all. At the horizon everything would appear to us to be stationary. It would take an infinite time for anything to happen at the horizon. Nevertheless all parts of this space-time are on the same footing, so that if we could reach the horizon we would find everything going on there in a normal fashion. But the place we had left would now form part of our new horizon, and it would appear to us now that everything was stationary there. Also, a ray of light could not perform the journey to the horizon and back in a finite time. Thus, on De Sitter's theory, unlike Einstein's, a ray of light could not go round the world. On Einstein's theory it is possible that some of the stars we see are mere ghosts, images of real stars that were there 1000 or 2000 or 3000 and so on million years ago. For the light rays sent out by a star would go all round the world in 1000 million years, and come together again at the place they left, forming there an image of the star that originally sent them out. On De Sitter's theory this could not happen. It is not very likely to happen, even on Einstein's theory, for the rays of light would

gradually become scattered by passing through gravitational fields on their way round the world, and it would only be quite exceptionally that they would unite again sufficiently well to produce an image.

Phil.: Is there any evidence at all for these theories?

Math. Phys.: Well, you probably know that the spiral nebulæ are supposed by many astronomers to be stellar universes like our own, and to be the most distant objects in the heavens. Now it is a puzzling fact that a large majority of these objects whose motions have been measured are moving away from us, and with velocities that are quite exceptionally high. We would naturally expect that about as many would be approaching us as are going away. The fact that a large majority are receding points to some systematic cause. This cause must be something quite different from anything we know to influence bodies scattered throughout so immense a volume of space. Now the radial motions of these spiral nebulæ are deduced, of course, from spectroscopic observations. The spectrum lines

of the receding nebulæ appear, compared with the lines given out by the same elements on earth, to be displaced towards the red end of the spectrum. From this displacement the velocity of recession is calculated. But might it not be that these lines are displaced because the atoms in those distant bodies are vibrating more slowly than similar atoms on earth? This slowness of vibration would give a displacement towards the red, and render it unnecessary to suppose that the nebulæ were receding. As I have said, in De Sitter's universe, everything would appear to be going more slowly the farther it was away. The phenomena presented by the spiral nebulæ, therefore, may indicate, not that they are receding, but the slowing down of all natural processes that De Sitter's theory would lead us to expect. I may mention that, besides this effect, we should expect an actual receding motion on De Sitter's theory, for in his universe a number of particles initially close together will tend to scatter.

Ord. Int. Per.: Does Einstein's theory account for the motions of the spiral nebulæ?

Math. Phys.: No. On the other hand it offers

hope of being able to account for that very large number, 3.10^{42}, among the constants of nature — a number which is unique amongst natural constants. But it may be that we are not compelled definitely to choose between these two theories. De Sitter's solution seems to apply to an empty world. Einstein's solution, on the other hand, applies to a world containing very much more matter than astronomers yet recognise. Both solutions, it must be remembered, are idealised solutions, neglecting irregularities due to the concentration of matter into stellar systems. Einstein's solution applies strictly only to a uniform distribution of matter throughout space. It is possible to regard the two theories as at opposite extremes. The real form of the universe may be something intermediate.

Phil.: Then De Sitter's solution, since it is for a world empty of matter, certainly supposes that space-time can exist independently of matter.

Math. Phys.: Even that is not certain. The mathematical expression used by De Sitter has a "singularity" at an immense distance. A singularity in the expression for *ds* may denote the

presence of matter. On the other hand it may not. If this singularity in De Sitter's formula denotes matter then De Sitter's solution supposes that a " mass-horizon " exists, and he is not really dealing with an empty world. But inasmuch as these questions are highly speculative I do not propose to discuss them at greater length.

SIXTH DIALOGUE

WORLD-BUILDING

Math. Phys.: I have so far discussed with you some of the consequences that flow from postulating a fundamental relation, the interval relation, between pairs of indefinables called point-events. The aspect of the interval relation with which we are concerned can be given mathematical expression — that is why, as physicists, we are concerned with it. This mathematical expression can be given a geometrical interpretation — it can be supposed to represent distance or, more generally, a spatio-temporal relation compounded of distance and time-lapse, between two neighbouring point-events. Point-events thus become entities which exist at a place at a moment. The properties of the space-time continuum so imported into our theory have been discussed. You observe that the space-time so introduced arises from giving the interval relation a geometrical

interpretation. Whether it is *necessary,* owing to the constitution of our minds, to give this interpretation to the interval, I do not propose to discuss. It may be that space and time can be introduced in another way, come from different roots, as it were, and that a geometrical interpretation of the interval is not necessary. Orthodox relativity theory, however, assumes that the geometrical interpretation is the natural interpretation. We have seen that the space-time continuum so derived is non-Euclidean. If we assume that it *is* Euclidean, then, in order to explain the observed motions of bodies moving freely in this space-time we have to introduce a gravitational force. If we recognise that the continuum is non-Euclidean, then there is no need of the gravitational force. Now let us see exactly what we have done. We have proved that if space-time is non-Euclidean and if we assume that it is Euclidean we shall *have* to introduce a gravitational force. There is a real vital connection between our conception of force and the non-Euclidean properties of space-time. In other words, the non-Euclidean properties of

space-time, owing to our persistence in using co-ordinates only appropriate to a Euclidean continuum, must manifest themselves to us as gravitational force. I now wish to discuss with you a yet further extension of the non-Euclidean geometry attributed to the continuum, and by doing so I shall give a geometrical interpretation of the forces we have not yet considered, namely, the electromagnetic forces. But I must warn you that this is a merely hypothetical interpretation. I can show that a continuum, possessing certain geometrical properties, can account for electro-magnetic phenomena. But I cannot show that it *must* do so. Therefore the more complicated continuum I am about to describe to you may be regarded as a graphical representation of the world, and not as the actual geometrical configuration which we apprehend as the world.

Phil.: You are going, I suppose, to show that certain geometrical properties of space-time correspond to electro-magnetic quantities, just as you showed that certain other geometrical properties of space-time correspond to material quantities such as density, momentum, and so on.

Math. Phys.: Yes, but with the difference that the geometry I am now about to discuss is not to be considered the natural geometry of space-time. You may regard this more general geometry that I am about to describe to you merely as a suggestive graphical representation of phenomena. The non-Euclidean geometry I have hitherto discussed is to be taken more seriously. I believe it to be accurately descriptive of the behaviour of our measuring appliances. We will call this geometry Riemann's geometry; it is, I believe, the actual geometry of the world. We will call the more general geometry I am about to describe Weyl's geometry.

Ord. Int. Per.: But if Riemann's geometry includes within it all non-Euclidean geometries, how can Weyl's geometry be something different?

Math. Phys.: It is different in the sense of being more general even than Riemann's geometry. It includes Riemann's geometry as a special case, just as Riemann's geometry includes Euclid's as a special case. Euclid's geometry rests on certain assumptions which are not logically necessary. Riemann's geometry, also, it can be shown, con-

tains an assumption which is not logically necessary. By removing this restriction we obtain Weyl's geometry.

Phil.: Can the process be continued? Is there nothing in Weyl's geometry but what is logically necessary?

Math. Phys.: The process can be continued. I may refer to that fact later. But we will go one step at a time. I will begin by calling your attention to an important distinction between Riemannian and Euclidean geometry. A vector, as you know, is a quantity which has *direction* and *length* or *magnitude*. To specify a velocity, for instance, it is not sufficient to give its magnitude; you must also give its direction. Temperature, on the other hand, does not have the idea of direction associated with it. We specify a temperature by saying that it is such and such a figure on such and such a scale. Force is another vector quantity. Force has direction as well as magnitude. Vector quantities, therefore, are well represented graphically by a line, since a line has a certain length and is in a certain direction. Now I want you to consider what

happens to a line, in a Euclidean plane, when it is taken round a closed circuit in such a way that it always remains parallel to its original direction. When it comes back to its starting point it has obviously experienced no change whatever. It has the same length and the same direction that it had to begin with. This may seem too obvious to be worth talking about. But suppose that, instead of a Euclidean plane surface, we are dealing with the surface of a sphere, and that we mark out a closed circuit on this surface. What is the analogous operation, on this surface, to the operation we have just performed? How can we move a line round a closed circuit on a spherical surface so that it is always parallel to its original direction? You will see, if you try to picture it, that, on a spherical surface, we cannot keep this Euclidean notion of parallel direction at all. How are we to proceed, therefore, if we wish the line, on returning to its starting point, to have the same direction it had at first? What happens, for instance, when a ship describes a closed circuit at sea — for the sea may be regarded as part of the surface of a sphere — and finally

points in the same direction it had to begin with?

Ord. Int. Per.: Well, its total change of direction must be four right angles. As it goes round the circuit it changes its direction constantly, but these changes bring it round to its original position if they all add up to four right angles.

Math. Phys.: There you are wrong. Remember that I am talking about the surface of a sphere. I have already told you that the geometry of a spherical surface is intrinsically different from the geometry of a Euclidean plane. The sum of the exterior angles of a triangle in a Euclidean plane is four right angles, but this is not the case with a spherical triangle. By making the ship change its direction through four right angles you do not bring it back to its original direction.

Ord. Int. Per.: Well, then, turn it through whatever total angle you have to turn it through in order to get it back to its original direction. It is perfectly obvious that a ship *can* get back to its original position on completing its circuit, and it must turn through some total angle to do

it. As you know the geometry of a sphere, you know what that angle must be.

Math. Phys.: Yes, but the difficulty is this. The total angle through which it must have turned will depend upon what circuit it has gone round. I cannot tell you what total angle it must have turned through to get back to its original direction unless you tell me what route it has followed. For a Euclidean plane I can say that the total angle, whatever the circuit, must be four right angles. I can enunciate no such rule for the sphere.

Ord. Int. Per.: Let us suppose we have two ships, originally side by side, and pointing in the same direction. They are sent on different closed circuits and are instructed to turn through the same total angles. Then they will no longer coincide in direction when they get back to their starting point?

Math. Phys.: Exactly. They obey the same rule for their motions, but their final directions depend on the routes they have followed. Now in general Riemannian geometry we have a similar state of affairs. Suppose that, in a general

Riemannian space, a curved space, we wish to move a vector from a point A to a point B. We must move it according to some rule, of course. The rule we shall adopt is the rule which, in Euclidean space, would leave the direction of the vector unchanged. We shall call movement according to this rule parallel displacement. The vector is moved, obeying this rule, from point A to point B. Now suppose we have another vector at the point A and that this vector originally coincided with the first vector. We move this second vector, obeying the same rule, to the point B. But we move it by a different route from that followed by the first vector. We shall find that, at B, the two vectors are pointing in different directions. It is on this fact that the gravitational field depends. It is owing to the fact that our space-time is of this general Riemannian kind that we perceive a gravitational force.

Phil.: You mean that that property of space-time that manifests to us as a gravitational field would produce this curious effect on the direction of a vector that you have described?

Math. Phys.: Yes.

Phil.: And you are going to suggest that some analogous property of space-time manifests to us as an electro-magnetic field?

Math. Phys.: Precisely. But remember always that I am now talking about a purely conceptual space-time governed by the geometry I am about to describe. You will observe that, in what I have said to you, I have said nothing about the vectors, taken from A to B by different routes, disagreeing as to length. They disagree as to direction, but they agree as to length. This is the Riemannian assumption that I spoke about. It is not a *necessary* assumption. Weyl's geometry dispenses with it. Weyl supposes that two vectors, originally coincident, and moved by parallel displacement to the same point, will differ not only in direction but also in length.

Phil.: So that on this new geometry you cannot attach any definite meaning to the assertion that you have the same standard of length at different places.

Math. Phys.: Exactly.

Phil.: Then what becomes of your measurements of length? If you have a standard of

length at one place its value at another place will depend on the route by which you took it there.

Math. Phys.: Yes. And I must remind you that it is a four-dimensional route that I am talking about. I am suggesting, for instance, that two equal foot-rules carried from London to New York, the one by aeroplane and the other by train and steamer, need not agree when they are placed side by side in New York. They have performed the same journey in space, but their time journeys have been different. Their four-dimensional routes are different. To deal with this ambiguity of length we have to do something analogous to what we did for the ambiguity of direction. In Riemannian geometry we lay down a system of co-ordinates at every point. In Weyl's geometry, besides doing this, we lay down a definite *gauge* at every point, that is, we arbitrarily erect at every point a standard of unit length. In flat space-time we can talk of the *same* co-ordinates at two separate points A and B. In curved space-time we cannot unambiguously define what co-ordinates at B are the same as the co-ordinates at A. Similarly,

in the more general space-time we are now considering we cannot define unambiguously the *same* length at A and B. Now the geometry of Weyl's space-time requires more quantities to determine it than does Riemannian geometry. We have seen that the metric properties of any region of space-time which is governed by Riemannian geometry can be specified when we know the quantities we have called the g's. We have also seen that there are ten of these quantities for any given region of space-time. These quantities, in defining the metric, also define the gravitational field. Now it can be shown that, in order to define the gauging system throughout a region we require four more quantities. What do these quantities correspond to in the world of phenomena?

Phil.: You mean that just as the ten geometrical quantities, the ten g's, correspond to ten quantities which define the gravitational field, so these other four geometrical quantities should correspond to some other physical phenomenon?

Math. Phys.: Precisely. And the identification we are to make is at once suggested by the

fact that the electro-magnetic field in a region is defined by four quantities called the electro-magnetic potentials.

Phil.: The cases do not seem to me quite parallel, nevertheless. Gravitational force exists in every region of space-time, but surely there are many regions where no electro-magnetic force exists.

Math. Phys.: That is so. Let us call the four geometrical quantities we have obtained K_1, K_2, K_3, K_4. I say that these correspond to the four electro-magnetic potentials. Now potentials are not forces. Forces are derived from potentials. If I want to know what electro-magnetic forces are due to certain potentials I perform certain mathematical operations on the potentials and the resulting expressions give me the values of the forces. Suppose no electro-magnetic forces exist in the region I am considering. Then these mathematical operations, performed on the potentials characteristic of that region, give zero as the value of the forces. Now these same mathematical operations performed on the K's for that region give a result which shows that, for that

region, comparisons of length are *not* ambiguous. So that two vectors moved from a point A to a point B within the region will not disagree in length, whatever routes they are moved by, if there are no electro-magnetic forces in that region. Thus we can say that that geometrical property of space-time which makes length comparison an ambiguous procedure is the property which manifests to us as electro-magnetic force.

Phil.: There seems to me to be a serious objection to the whole of this point of view. In the last dialogue you explained to us that a material measuring standard at any place took up the size it possesses there by adjustment to the curvature of space-time. The radius of an electron, for example, is always a definite fraction of the radius of curvature of space at that place. This means, surely, that you already have a natural gauge everywhere.

Math. Phys.: You are quite right. But you must remember that I introduced this more general geometry of Weyl purely as a conceptual geometry. I am not suggesting that its properties are the properties of actual space-time.

Phil.: But why not? Your identification of electro-magnetic forces with geometrical quantities seems to me just as plausible as your previous identification of gravitational forces with geometrical quantities.

Math. Phys.: The reason is this. In geometrising gravitation I am talking all the time about actual physical processes. In talking about length ambiguity I am not. The operation of parallel displacement of a vector is not a description of the physical procedure by which lengths are compared at different places.

Ord. Int. Per.: Then what is the point of discussing it?

Math. Phys.: Well, it is a step towards a yet wider generalisation. We have begun by postulating a relation-structure as the substratum from which we wish to derive the world. We began by making this structure Riemannian, and, so far as matter and gravitation are concerned, we derived the world we know. It is interesting to know whether the world could be derived from a still more general relation-structure. One of the aims of the theory of relativity is to express

laws of nature in such a way that these laws preserve the same form for every observer, whatever his state of motion. Equations that obey this criterion are called tensor equations. They are unaffected, we may say, by any change of co-ordinates. But we have seen that we can imagine a still more general relation-structure of the world where the gauge-system, as well as the co-ordinate-system employed, may be varied. It suggests itself, therefore, that we may be able to derive quantities from this more general structure which are independent not only of changes of co-ordinates but of changes of gauge. This can be done, and it can be done for a relation-structure even more general than that postulated by Weyl. Eddington has assumed a relation-structure which, expressed geometrically, dispenses with a restriction which exists in Weyl's geometry. Weyl admits that a vector, on being moved by parallel displacement, may change its length as well as its direction. But he assumes that this change of length is independent of the original direction of the vector. Eddington dispenses with this restriction, and

thus arrives at what appears to be almost the minimum of relation-structure conceivable. Yet even from this comparatively amorphous substratum of the world he manages to derive expressions which are related in the way that the physical quantities of experience are related.

Phil.: The laws of nature, then, tell us comparatively little about the structure of the world? Provided *some* structure exists, the world must be as it is?

Math. Phys.: Yes, given the fact that our minds are of a particular kind. The relation-structure could be different, within pretty wide limits, without producing a different perceptual universe. Our minds select certain aspects of what exists (in the logical sense) and pay attention only to those.

Ord. Int. Per.: Then the laws of nature really only express the way in which the mind arranges the material that is, as it were, offered to it?

Math. Phys.: That is largely true. But in taking leave of you I wish to point out that, complete as relativity theory appears to be, the science of physics has by no means reached

finality. We have been considering large scale phenomena. Phenomena of a quite different kind meet us when we investigate atomic and sub-atomic structures. Relativity theory, so far, has thrown no light on these phenomena. The general theory of relativity is, I believe, in its comprehensiveness and profundity, the greatest scientific discovery of which we have any record. We may be thankful that it is not sufficient. The universe remains mysterious. Our search must go on.

MATHEMATICAL APPENDIX

THE NEWTONIAN TRANSFORMATION

In the Newtonian scheme two systems of reference are considered to be equivalent if they are connected by the equations $x' = x - vt$, $y' = y$, $z' = z$, $t' = t$. The physical meaning of these symbols was supposed to be that x, y, z, t were the measured lengths and times in one system, and x', y', z', t' the measured lengths and times in a system whose axes are parallel to those of the first, and which is moving in the direction of the x – axis with a velocity v. According to the theory of relativity this assumption is unjustified.

The fundamental equation of Newtonian dynamics is

$$\text{Force} = \text{Mass} \times \text{Acceleration}.$$

Mass is an invariant scalar quantity. This being so, Force is invariant for the systems connected by the equations given above. For

$$\frac{dx'}{dt'} = \frac{dx}{dt} - v, \qquad \frac{dy}{dt'} = \frac{dy}{dt} \qquad \frac{dz'}{dt'} = \frac{dz}{dt}$$

and

$$\frac{d^2x'}{dt'^2} = \frac{d^2x}{dt^2} \qquad \frac{d^2y'}{dt'^2} = \frac{d^2y}{dt^2} \qquad \frac{d^2z'}{dt'^2} = \frac{d^2z}{dt^2}$$

Not velocity, but acceleration, is invariant. Therefore force is invariant. The addition theorem for velocities is given by the equation

$$\frac{dx'}{dt'} = \frac{dx}{dt} - v \text{ or } q' = q - v.$$

THE LORENTZ TRANSFORMATION

Putting $i\tau = t$ we may express the interval in terms of three spatial measurements and one imaginary time measurement. It becomes $ds^2 = dx^2 + dy^2 + dz^2 + d\tau^2$. This expression is perfectly homogeneous. Minkowski showed that the Lorentz transformation may be obtained from the formulæ for the rotation of rectangular axes in this four-dimensional (space and imaginary time) continuum. A rotation through an angle θ in the $x\tau$ plane is given by $x = x' \cos\theta - \tau' \sin\theta$, $y = y'$, $z = z'$, $\tau = x' \sin\theta + \tau' \cos\theta$. Put $v = i \tan\theta$. Then $\cos\theta = (1 - v^2)^{-\frac{1}{2}} = \beta$, and $\sin\theta = iv\beta$. Hence the formulæ for rotation become

$$x = \beta(x' - iv\tau'),\ y = y',\ z = z',\ \tau = \beta(\tau' + ivx')$$

Remembering that $i\tau = t$ and $i\tau' = t'$, these equations become those of the Lorentz transformation

$$x = \beta(x' - vt'),\ y = y',\ x = z',\ t = \beta(t - vx').$$

Making the substitutions we have

$$ds^2 = dx^2 + dy^2 + dz^2 - dt^2 = dx'^2 + dy'^2 + dz'^2 - dt'^2$$

Apart from obvious changes, such as a displacement of the origin or a rotation about the time axis, the Lorentz transformation is the only one which leaves the form of ds unaltered. The Lorentz transformation may be deduced also from the condition that the velocity of light is to be the same for two observers in uniform translatory motion.

THE PRINCIPLE OF EQUIVALENCE

In flat space-time co-ordinates x, y, z, t may be chosen so that the expression for the interval has the form

$$ds^2 = dt^2 - dx^2 - dy^2 - dz^2 \qquad (1)$$

By making the transformation of co-ordinates

$$\begin{aligned} t &= f_1(x_1, x_2, x_3, x_4) \\ x &= f_2(x_1, x_2, x_3, x_4) \\ y &= f_3(x_1, x_2, x_3, x_4) \\ z &= f_4(x_1, x_2, x_3, x_4) \end{aligned}$$

the formula for ds assumes the form

$$\begin{aligned} ds^2 = {} & g_{11}dx_1^2 + g_{22}dx_2^2 + g_{33}dx_3^2 + g_{44}dx_4^2 \\ & + 2g_{12}dx_1dx_2 + 2g_{13}dx_1dx_3 + 2g_{14}dx_1dx_4 \\ & + 2g_{23}dx_2dx_3 + 2g_{24}dx_2dx_4 + 2g_{34}dx_3dx_4 \end{aligned} \quad (2)$$

where

$$g_{11} = \left(\frac{\delta f_1}{\delta x_1}\right)^2 + \left(\frac{\delta f_2}{\delta x_1}\right)^2 + \left(\frac{\delta f_3}{\delta x_1}\right)^2 + \left(\frac{\delta f_4}{\delta x_1}\right)^2 \text{ etc.}$$

and

$$g_{12} = \frac{\delta f_1}{\delta x_1}\frac{\delta f_1}{\delta x_2} + \frac{\delta f_2}{\delta x_1}\frac{\delta f_2}{\delta x_2} + \frac{\delta f_3}{\delta x_1}\frac{\delta f_3}{\delta x_2} + \frac{\delta f_4}{\delta x_1}\frac{\delta f_4}{\delta x_2} \text{ etc.}$$

The equations to a straight line, that is, to the unconstrained motion of a body in the x, y, z, t co-ordinates, are comprised in the condition $\int ds$ is stationary. This condition will also give the path of the body, that is, a geodesic, in the co-ordinates x_1, x_2, x_3, x_4. We assume that it continues to do so in space-time which is not flat, that is, is not such that the expression for ds can be reduced, by a suitable choice of co-ordinates, to the form (1). This generalisation is an example of the *principle of equivalence.*

Similarly, since $ds = 0$ is the equation for a ray of light in system (1) and in any system derivable from (1) by mathematical transformations of co-ordinates, we shall assume that it holds good for

light propagated in a region of space-time where the formula for ds is not deducible from (1) by mathematical transformations. But the principle of equivalence is not supposed to hold universally. Equations involving higher derivatives of the g's have an essential reference to the curvature of the space-time considered. For such expressions, therefore, one may not assume that the curvature makes no difference, and the principle of equivalence may not be applied.

THE TENSOR CALCULUS

Definitions.

A Tensor is a set of numbers associated with a point and dependent upon a co-ordinate system in such a way that when the co-ordinate system is changed the numbers change in accordance with the transformation laws to be defined.

Consider first a tensor of the first rank. (We shall deal throughout with a four-dimensional space.) A tensor of the first rank is a set of four numbers. These numbers are called its components. Let the components of such a tensor, in a given system of co-ordinates, be A^1, A^2, A^3,

A^4. This set of four numbers is associated with a point x_1, x_2, x_3, x_4. We may consider them, if we like, as the components of a force at that point. A change of the system of co-ordinates is made —

$$\begin{aligned} x'_1 &= f\ (x_1, x_2, x_3, x_4) \\ x'_2 &= \varphi\ (x_1, x_2, x_3, x_4) \\ x'_3 &= \chi\ (x_1, x_2, x_3, x_4) \\ x'_4 &= \psi\ (x_1, x_2, x_3, x_4) \end{aligned}$$

The set of numbers A^1, A^2, $A,^3$ A^4 are said to be the components of a *contravariant* tensor of the first rank if, in the (x'_1, x'_2, x'_3, x'_4) system of co-ordinates they assume the values

$$A'^1 = \frac{\delta f}{\delta x_1} A^1 + \frac{\delta f}{\delta x_2} A^2 + \frac{\delta f}{\delta x_3} A^3 + \frac{\delta f}{\delta x_4} A^4$$

$$A'^2 = \frac{\delta \varphi}{\delta x_1} A^1 + \frac{\delta \varphi}{\delta x_2} A^2 + \frac{\delta \varphi}{\delta x_3} A^3 + \frac{\delta \varphi}{\delta x_4} A^4$$

$$A'^3 = \frac{\delta \chi}{\delta x_1} A^1 + \frac{\delta \chi}{\delta x_2} A^2 + \frac{\delta \chi}{\delta x_3} A^3 + \frac{\delta \chi}{\delta x_4} A^4$$

$$A'^4 = \frac{\delta \psi}{\delta x_1} A^1 + \frac{\delta \psi}{\delta x_2} A^2 + \frac{\delta \psi}{\delta x_3} A^3 + \frac{\delta \psi}{\delta x_4} A^4$$

This transformation law may be summarised —

$$A'^\mu = \frac{\delta x'^\mu}{\delta x_\alpha} A^\alpha \qquad (1)$$

where, corresponding to each value of μ, we sum the terms corresponding to $\alpha = 1, 2, 3, 4$.

A set of four numbers A_1, A_2, A_3, A_4, is said to be the components of a *covariant* tensor of the first rank if, in changing from the system (x_1, x_2, x_3, x_4) to the system (x'_1, x'_2, x'_3, x'_4) they assume the values —

$$A'_\mu = \frac{\delta x_\alpha}{\delta x'_\mu} A_\alpha \tag{2}$$

where, corresponding to each value of μ, we sum the terms corresponding to $\alpha = 1, 2, 3, 4$.

To distinguish contravariant from covariant tensors it is usual to place the suffix for a contravariant tensor above and for a covariant tensor below. Thus A^μ will represent a contravariant tensor of the first rank and A_μ a covariant tensor of the first rank.

Tensors of the second rank have sixteen components. These components, for a contravariant tensor, are represented by $A^{\mu\nu}$, and are obtained by giving μ and ν independently all values from 1 to 4.

The transformation law for a contravariant tensor of the second rank is —

$$A'^{\mu\nu} = \frac{\delta x'_\mu}{\delta x_\alpha} \frac{\delta x'_\nu}{\delta x_\beta} A^{\alpha\beta} \tag{3}$$

and for a covariant tensor of the second rank is

$$A'_{\mu\nu} = \frac{\delta x_\alpha}{\delta x'_\mu} \frac{\delta x_\beta}{\delta x'_\nu} A_{\alpha\beta} \tag{4}$$

A *mixed* tensor of the second rank obeys the transformation law —

$$A'^{\nu}_{\mu} = \frac{\delta x_\alpha}{\delta x'_\mu} \frac{\delta x'_\nu}{\delta x_\beta} A^{\beta}_{\alpha} \tag{5}$$

It will be noticed that the same suffix occurring twice in an expression denotes summation. This will always be the case unless the contrary is stated. Thus —

$A_\mu B^\mu$ denotes $A_1B^1 + A_2B^2 + A_3B^3 + A_4B^4$.

Any such suffix is a dummy suffix, for it is obvious that the letter μ in the above example may be replaced by any other since it runs through all the values from 1 to 4.

The laws of transformation for tensors of higher rank are easily deduced from the above cases.

Thus —

$$A'^{\tau}_{\mu\nu\sigma} = \frac{\delta x_\alpha}{\delta x'_\mu} \frac{\delta x_\beta}{\delta x'_\nu} \frac{\delta x_\gamma}{\delta x'_\sigma} \frac{\delta x'_\tau}{\delta x_\delta} A^{\delta}_{\alpha\beta\gamma}$$

This is a tensor of the fourth rank and possesses 256 components. Each component in the accented system is the sum of 256 terms.

We contemplate only continuous changes of co-ordinates, so that the differential coefficients appearing in these equations are all finite. From this it follows that if all the components of a tensor in one system of co-ordinates are zero, they are zero in all systems of co-ordinates.

In this property lies the importance of the tensor calculus in relativity theory. We wish to find equations which hold good in all systems of co-ordinates, that is, we wish to express the equations occurring in our investigation as tensor equations. For if we have a tensor equation $U^{\mu\nu} = V^{\mu\nu}$ the equation $U^{\mu\nu} - V^{\mu\nu} = 0$, which is also a tensor equation, holds good in all systems of co-ordinates, and therefore $U^{\mu\nu} = V^{\mu\nu}$ holds good in all systems of co-ordinates.

It is obvious that the co-ordinate differences between any two infinitely near points form a contravariant tensor of the first rank, for they obey the transformation law —

$$dx'_\mu = \frac{\delta x'_\mu}{\delta x_\alpha} dx_\alpha$$

For this reason it is sometimes convenient, in order to show the contravariant character, to write $dx_\mu = (dx)^\mu$.

Multiplication.

The expression $A\ B_\nu$ does not represent a summation, but the sixteen components of a mixed tensor of the second rank, namely, A^1B_1, $A^1B_2 \ldots A^4B_3$, A^4B_4. The expression $A^\mu B_\mu$, on the other hand, represents, as we have seen, the sum of four quantities. This expression $A^\mu B_\mu$ is called the *inner product* of the two tensors of the first rank, A^μ, B_ν, or the *contraction* of the mixed tensor $A^\mu B_\nu$. Mixed tensors of any rank may be contracted. Thus the mixed tensor $A^\alpha_{\beta\gamma}$ may be contracted to $A^\alpha_{\beta\alpha}$, which consists of four components each the sum of four terms, as $A^1_{11} + A^2_{12} + A^3_{13} + A^4_{14}$. In this way, from the mixed tensor of the third rank A^α_β a covariant tensor of the first rank is formed. The rank is reduced by two. Similarly, the mixed tensor of the second rank A^μ_ν has its rank reduced by two on contraction, and becomes A^μ_μ, a *scalar* or *invariant*. It has, of course, the same value in all co-ordinate systems.

Similarly, $A^{\mu\nu}_{\mu\nu}$, $A^{\nu}_{\mu}B^{\mu}_{\nu}$ are invariants. The upper and lower suffixes cancel out, as it were.

The Rigorous Quotient Theorem.

A quantity which, on inner multiplication by any covariant or any contravariant tensor of the first rank always gives a tensor, is itself a tensor. For suppose that, whatever contravariant tensor B^{ν} may be, $A(\mu\nu)B^{\nu}$ is always a covariant tensor. Then, by (2) —

$$A'(\mu\nu)B'^{\nu} = \frac{\delta x_{\alpha}}{\delta x'_{\mu}}\left[A(\alpha\beta)B^{\beta}\right]$$

But, by using the reverse of (1) —

$$B^{\beta} = \frac{\delta x_{\beta}}{\delta x'_{\nu}} B'^{\nu}$$

Hence, substituting for B^{β} in the above equation

$$B'^{\nu}\left(A'(\mu\nu) - \frac{\delta x_{\alpha}}{\delta x'_{\mu}}\frac{\delta x_{\beta}}{\delta x'_{\nu}} A(\alpha\beta)\right) = 0$$

But B'^{ν} is arbitrary. Therefore the expression in brackets must be identically zero. Hence —

$$A'(\mu\nu) = \frac{\delta x_{\alpha}}{\delta x'_{\mu}}\frac{\delta x_{\beta}}{\delta x'_{\nu}} A(\alpha\beta)$$

and by comparing with (4) we see that $A(\mu\nu)$ is a covariant tensor of the second rank.

Fundamental Tensors.

Consider the expression for the interval —

$$ds^2 = g_{\mu\nu} dx_\mu dx_\nu \tag{6}$$

The expression ds^2 is an invariant or tensor of zero rank. Since dx_ν is an arbitrary contravariant tensor of the first rank it follows that $g_{\mu\nu} dx_\mu$ is a covariant tensor of the first rank. Therefore, since dx_μ is an arbitrary contravariant tensor of the first rank it follows that $g_{\mu\nu}$ is a covariant tensor of the second rank.

Form the determinant of the fourth order from the components of $g_{\mu\nu}$. Call this determinant g. Take the minor of any element $g_{\alpha\beta}$ of this determinant and divide it by g. Call this quotient $g^{\alpha\beta}$. Consider the expression $g_{\mu\sigma} g^{\nu\sigma} = g_\mu^\nu$.

From a known property of determinants —

$$g_\mu^\nu = 1 \text{ if } \mu = \nu \text{ and } g_\mu^\nu = 0 \text{ if } \mu \pm \nu.$$

Hence $g_\mu^\nu A^\mu = A^\nu$.

Since A^μ is an arbitrary tensor of the first rank g_μ^ν is a tensor. Now since $g_{\mu\sigma} A^\mu$, when A^μ is an arbitrary contravariant tensor of the first rank, is an arbitrary covariant tensor of the first rank,

and since $g_{\mu\sigma}g^{\nu\sigma}A^{\mu} = g_{\mu}A^{\mu} = A^{\nu}$, it follows that $g^{\nu\sigma}$ is a contravariant tensor of the second rank.

Thus we have three fundamental tensors —

$$g_{\mu\nu},\ g^{\mu\nu},\ g_{\mu}^{\nu}.$$

The operation of raising a suffix is defined by —

$$A^{\mu} = g^{\mu\nu}A_{\nu}.$$

Of lowering a suffix by —

$$A_{\mu} = g_{\mu\nu}A^{\nu}.$$

Similar definitions apply to tensors of any order.

Symmetrical and Anti-symmetrical Tensors.

A tensor is said to be symmetrical in the suffixes μ, ν, if $A^{\mu\nu} = A^{\nu\mu}$ or $A_{\mu\nu} = A_{\nu\mu}$.

It is said to be anti-symmetrical if $A^{\mu\nu} = -A^{\nu\mu}$, or $A_{\mu\nu} = -A_{\nu\mu}$.

For an anti-symmetrical tensor we see that the four components for which $\mu = \nu$ vanish. The other twelve form six pairs, the members of a pair being equal and of opposite signs. Thus an anti-symmetrical tensor of the second rank has only six components numerically different. The anti-symmetrical tensor of the third rank $A^{\mu\nu\sigma}$ has only four numerically different components.

The anti-symmetrical tensor of the fourth rank $A^{\mu\nu\sigma\tau}$ has only one component. Anti-symmetrical tensors of rank higher than the fourth cannot occur in a four-dimensional continuum.

The tensors $g_{\mu\nu}$, $g^{\mu\nu}$, are symmetrical. A contravariant tensor of the first rank is what is usually called a vector. The term vector may be extended to denote also a covariant tensor of the first rank. Two vectors, A_μ, B_μ, are said to be perpendicular if $A_\mu B^\mu = 0$.

In rectangular co-ordinates $A_\mu B^\mu$ is the scalar product of elementary vector theory. In all co-ordinates the length of a vector A_μ is defined to be $l^2 = A_\mu A^\mu$.

A vector is self-perpendicular if its length vanishes. Since $g_{\mu\nu}(dx)^\mu = (dx)_\nu$, we may write the formula for the interval $ds^2 = (dx)_\nu (dx)^\nu$.

Length is, of course, an invariant. Thus the length of $A_{\mu\nu\sigma\tau}$, that is, $A_{\mu\nu\sigma\tau}\ A^{\mu\nu\sigma\tau}$ is an invariant.

From a tensor of even rank an invariant called its *Spur* may be constructed. Thus from $A_{\mu\nu\sigma\tau}$ we can form the tensor $A^{\sigma\tau}_{\mu\nu}$ and contract, obtaining $A^{\mu\nu}_{\mu\nu} = A$, the spur.

Equations of a geodesic.

We have to determine the equations from the condition that fds is stationary.

The beginning and end of the path being fixed, we give every intermediate point an arbitrary infinitesmal displacement δx_σ. We have

$$ds^2 = g_{\mu\nu} dx_\mu dx_\nu.$$

Hence

$$2ds\,\delta(ds) = dx_\mu dx_\nu \delta g_{\mu\nu} + g_{\mu\nu} dx_\mu \delta(dx_\nu) + g_{\nu\mu} dx_\nu \delta(dx_\mu)$$
$$= dx_\mu dx_\nu \frac{\delta g_{\mu\nu}}{\delta x_\sigma} \delta x_\sigma + g_{\mu\nu} dx_\mu d(\delta x_\nu) + g_{\mu\nu} dx_\nu d(\delta x_\mu)$$

The condition being $-f\delta(ds) = 0$,
we have

$$\frac{1}{2}\int \left\{ \frac{dx_\mu}{ds}\frac{dx_\nu}{ds}\frac{\delta g_{\mu\nu}}{\delta x_\sigma}\delta x_\sigma + g_{\mu\nu}\frac{dx_\mu}{ds}\frac{d}{ds}(\delta x_\nu) \right.$$
$$\left. + g_{\mu\nu}\frac{dx_\nu}{ds}\frac{d}{ds}(\delta x_\mu) \right\} ds = 0$$

Now $$g_{\mu\nu}\frac{dx_\mu}{ds}\frac{d}{ds}(\delta x_\nu)$$

may be written $$g_{\mu\sigma}\frac{dx_\mu}{ds}\frac{d}{ds}(\delta x_\sigma)$$

since the ν is a dummy suffix, and, similarly,

$$g_{\mu\nu}\frac{dx_\nu}{ds}\frac{d}{ds}(\delta x_\mu)$$

may be written $g_{\sigma\nu}\dfrac{dx_\nu}{ds}\dfrac{d}{ds}(\delta x_\sigma)$

Thus the above integral may be written —

$$\frac{1}{2}\int\left\{\frac{dx_\mu}{ds}\frac{dx_\nu}{ds}\frac{\delta g_{\mu\nu}}{\delta x_\sigma}\delta x_\sigma+\left(g_{\mu\sigma}\frac{dx_\mu}{ds}+g_{\sigma\nu}\frac{dx_\nu}{ds}\right)\times\frac{d}{ds}(\delta x_\sigma)\right\}ds=0$$

The second term within the brackets { } may be integrated by the method of partial integration, and since δx_σ vanishes at both limits the whole integral can be written —

$$\frac{1}{2}\int\left\{\frac{dx_\mu}{ds}\frac{dx_\nu}{ds}\frac{\delta g_{\mu\nu}}{\delta x_\sigma}-\frac{d}{ds}\left(g_{\mu\sigma}\frac{dx_\mu}{ds}+g_{\sigma\nu}\frac{dx_\nu}{ds}\right)\right\}\times\delta x_\sigma\, ds=0$$

Since this must hold for the *arbitrary* displacement δx_σ the quantity in brackets { } must vanish at all points of the path. Hence —

$$\frac{1}{2}\frac{dx_\mu}{ds}\frac{dx_\nu}{ds}\frac{\delta g_{\mu\nu}}{\delta x_\sigma}-\frac{1}{2}\frac{dg_{\mu\sigma}}{ds}\frac{dx_\mu}{ds}-\frac{1}{2}\frac{dg_{\sigma\nu}}{ds}\frac{dx_\nu}{ds}$$

$$-\frac{1}{2}g_{\mu\sigma}\frac{d^2x_\mu}{ds^2}-\frac{1}{2}g_{\sigma\nu}\frac{d^2x_\nu}{ds^2}=0$$

Now $\dfrac{dg_{\mu\sigma}}{ds}=\dfrac{\delta g_{\mu\sigma}}{\delta x_\nu}\dfrac{dx_\nu}{ds}$ and $\dfrac{dg_{\sigma\nu}}{ds}=\dfrac{\delta g_{\sigma\nu}}{\delta x_\mu}\dfrac{dx_\mu}{ds}$

Also $\frac{1}{2} g_{\mu\sigma} \frac{d^2x_\mu}{ds^2} = \frac{1}{2} g_{\epsilon\sigma} \frac{d^2x_\epsilon}{ds^2}$ and $\frac{1}{2} g_{\sigma\nu} \frac{d^2x_\nu}{ds^2} = \frac{1}{2} g_{\sigma\epsilon} \frac{d^2x_\epsilon}{ds^2}$

and since $g_{\epsilon\sigma} = g_{\sigma\epsilon}$, we have

$$\frac{1}{2} g_{\mu\sigma} \frac{d^2x_\mu}{ds^2} + \frac{1}{2} g_{\sigma\nu} \frac{d^2x_\nu}{ds^2} = g_{\epsilon\sigma} \frac{d^2x_\epsilon}{ds^2}$$

Thus the expression above becomes

$$\frac{1}{2} \frac{dx_\mu}{ds} \frac{dx_\nu}{ds} \left(\frac{\delta g_{\mu\nu}}{\delta x_\sigma} - \frac{\delta g_{\mu\sigma}}{\delta x_\nu} - \frac{\delta g_{\nu\sigma}}{\delta x_\mu} \right) - g_{\epsilon\sigma} \frac{d^2x_\epsilon}{ds^2} = 0$$

Multiply throughout by $-g^{\sigma\alpha}$, remembering that $g^{\sigma\alpha} g_{\epsilon\sigma} = g^\alpha_\epsilon$, and we obtain the equations

$$\frac{d^2x_\alpha}{ds^2} + \frac{1}{2} g^{\sigma\alpha} \left(\frac{\delta g_{\mu\sigma}}{\delta x_\nu} + \frac{\delta g_{\nu\sigma}}{\delta x_\mu} - \frac{\delta g_{\mu\nu}}{\delta x_\sigma} \right) \frac{dx_\mu}{ds} \frac{dx_\nu}{ds} = 0$$

These are the required equations to a geodesic.

Christoffel's 3-Index Symbols.

The expressions

$$[\mu\nu,\sigma] = \frac{1}{2} \left(\frac{\delta g_{\mu\sigma}}{\delta x_\nu} + \frac{\delta g_{\nu\sigma}}{\delta x_\mu} - \frac{\delta g_{\mu\nu}}{\delta x_\sigma} \right)$$

$$\{\mu\nu,\sigma\} = \frac{1}{2} g^{\sigma\lambda} \left(\frac{\delta g_{\mu\lambda}}{\delta x_\nu} + \frac{\delta g_{\nu\lambda}}{\delta x_\mu} - \frac{\delta g_{\mu\nu}}{\delta x_\lambda} \right)$$

are called the 3-index symbols of Christoffel. The equations just given for the geodesic may evidently be written

$$\frac{d^2x_\alpha}{ds^2} + \{\mu\nu,\alpha\}\frac{dx_\mu}{ds}\frac{dx_\nu}{ds} = 0 \qquad (7)$$

It is obvious from the definitions that

$$\{\mu\nu,\sigma\} = g^{\sigma\lambda}[\mu\nu,\lambda]$$

It can also be shown that $[\mu\nu,\sigma] = g_{\sigma\lambda}\{\mu\nu,\lambda\}$. For we have $g_{\sigma\alpha}\{\mu\nu,\sigma\} = g_{\sigma\alpha}g^{\sigma\lambda}[\mu\nu,\lambda] = g_\alpha^\lambda[\mu\nu,\lambda]$.

Now g_α^λ acts as a substitution operator on any expression (not necessarily a tensor). For all components of g_α^λ vanish except those where $\lambda = \alpha$, and they are equal to unity.

Thus

$$g_\alpha^\lambda[\mu\nu,\lambda] = [\mu\nu,\alpha]$$

and the theorem is proved.

From the definition of $[\mu\nu,\sigma]$ it follows that

$$[\mu\nu,\sigma] + [\sigma\nu,\mu] = \frac{\delta g_{\mu\sigma}}{\delta x_\nu}$$

There are forty different 3-index symbols of each kind. The 3-index symbols are not tensors.

Covariant Differentiation.

Consider the four quantities $\dfrac{\delta\varphi}{\delta x_\nu}$

where φ is an invariant function of position.

If we change the co-ordinate system to x'_ν these four quantities transform according to the law —

$$\frac{\delta\varphi}{\delta x'_\mu} = \frac{\delta x_\alpha}{\delta x'_\mu}\,\frac{\delta\varphi}{\delta x_\alpha}$$

Thus by differentiating an invariant we obtain a covariant vector. But by differentiating a vector we do not obtain a tensor. But we can obtain a tensor which plays the part of a differential coefficient and reduces to the ordinary differential coefficient in rectangular co-ordinates by the following method.

A "velocity" $\frac{dx_\mu}{ds}$ is a contravariant vector, since dx_μ is a contravariant vector and ds is invariant. Hence $A_\mu \frac{dx_\mu}{ds}$

is invariant where A_μ is any covariant vector.

And $$\frac{d}{ds}\left(A_\mu \frac{dx_\mu}{ds}\right)$$

is invariant along a geodesic, since a geodesic is defined independently of the co-ordinate system, and the expression gives the rate of change of an invariant per unit interval.

Performing the differentiation, we have —

$$\frac{\delta A_\mu}{dx_\nu}\frac{dx_\nu}{ds}\frac{dx_\mu}{ds} + A_\mu \frac{d^2x_\mu}{ds^2}$$

and from (7), since

$$A_\mu \frac{d^2x_\mu}{ds^2} = A_\alpha \frac{d^2x_\alpha}{ds^2} = -A_\alpha \{\mu\nu,\alpha\} \frac{dx_\mu}{ds}\frac{dx_\nu}{ds}$$

we have $\frac{dx_\mu}{ds}\frac{dx_\nu}{ds}\left(\frac{\delta A_\mu}{\delta x_\nu} - A_\alpha \{\mu\nu,\alpha\}\right)$ is invariant.

But in this result second order differentials (which distinguish the geodesic) do not occur, and the result is therefore general.

Since $\frac{dx_\mu}{ds}$ and $\frac{dx_\nu}{ds}$ are contravariant vectors the expression

$$A_{\mu\nu} = \frac{\delta A_\mu}{\delta x_\nu} - \{\mu\nu,\alpha\}\ A_\alpha \tag{8}$$

is a covariant tensor of the second rank. It is called the covariant derivative of A_μ. In rectangular co-ordinates, since the components of $g_{\mu\nu}$ are constants, the 3-index symbols vanish, and the covariant derivative reduces to the ordinary derivative.

The covariant derivative of A^μ is

$$A^\mu_\nu = \frac{\delta A^\mu}{dx_\nu} + \{\epsilon\nu,\mu\}\ A^\epsilon \tag{9}$$

We add the formulæ for covariant derivatives of tensors of the second rank.

$$A^{\mu\nu}_\sigma = \frac{\delta A^{\mu\nu}}{\delta x_\sigma} + \{\alpha\sigma,\mu\}\ A^{\alpha\nu} + \{\alpha\sigma,\nu\}\ A^{\mu\alpha} \tag{10}$$

$$A^\nu_{\mu\sigma} = \frac{\delta A^\nu_\mu}{\delta x_\sigma} - \{\mu\sigma,\alpha\}\ A^\nu_\alpha + \{\alpha\sigma,\nu\}\ A^\alpha_\mu \tag{11}$$

$$A_{\mu\nu\sigma} = \frac{\delta A_{\mu\nu}}{\delta x_\sigma} - \{\mu\sigma,\alpha\}\ A_{\alpha\nu} - \{\nu\sigma,\alpha\}\ A_{\mu\alpha} \tag{12}$$

If we apply the formula (12) to $g_{\mu\nu}$ we shall find that the covariant derivative of $g_{\mu\nu}$ vanishes identically. By this means, therefore, we cannot build up tensors of higher rank from the $g_{\mu\nu}$. But we can obtain a tensor of higher rank from the $g_{\mu\nu}$ by the following method. Let the covariant tensor of the second rank $A_{\mu\nu}$ in (12) be the covariant derivative (8) of A_μ. Then

$$A_{\mu\nu\sigma} = \frac{\delta}{\delta x_\sigma}\left(\frac{\delta A_\mu}{\delta x_\nu} - \{\mu\nu,\alpha\}\ A_\alpha\right) - \{\mu\sigma,\alpha\}\left(\frac{\delta A_\alpha}{\delta x_\nu} - \{\alpha\nu,\epsilon\}\ A_\epsilon\right) - \{\nu\sigma,\alpha\}\left(\frac{\delta A_\mu}{\delta x_\alpha} - \{\mu\alpha,\epsilon\}\ A_\epsilon\right)$$

$$= \frac{\delta^2 A_\mu}{\delta x_\sigma \, \delta x_\nu} - \{\mu\nu,\alpha\} \frac{\delta A_\alpha}{\delta x_\sigma} - \{\mu\sigma,\alpha\} \frac{\delta A_\alpha}{\delta x_\nu} - \{\nu\sigma,\alpha\}$$

$$\times \frac{\delta A_\mu}{\delta x_\alpha} + \{\nu\sigma,\alpha\} \{\mu\alpha,\epsilon\} A_\epsilon + \{\mu\sigma,\alpha\} \{\alpha\nu,\epsilon\} A_\epsilon$$

$$- A_\alpha \frac{\delta}{\delta x_\sigma} \{\mu\nu,\alpha\} \tag{13}$$

The last term of (13) may be written

$$- A_\epsilon \frac{\delta}{\delta x_\sigma} \{\mu\nu,\epsilon\}$$

since α is a dummy suffix. The last two terms of (13) may therefore be written

$$A_\epsilon \left(\{\mu\sigma,\alpha\} \{\alpha\nu,\epsilon\} - \frac{\delta}{\delta x_\sigma} \{\mu\nu,\epsilon\} \right) \tag{14}$$

We may now immediately write down the formula for $A_{\mu\sigma\nu}$ by interchanging σ and ν in (13). The sum of the first five terms of (13) is unchanged by the substitution. The remaining terms, by (14) become

$$A_\epsilon \left(\{\mu\nu,\alpha\} \{\alpha\sigma,\epsilon\} - \frac{\delta}{\delta x_\nu} \{\mu\sigma,\epsilon\} \right)$$

Therefore $A_{\mu\nu\sigma} - A_{\mu\sigma\nu} = A_\epsilon B^\epsilon_{\mu\nu\sigma}$, where

$$B^\epsilon_{\mu\nu\sigma} = \{\mu\sigma,\alpha\} \{\alpha\nu,\epsilon\} - \{\mu\nu,\alpha\} \{\alpha\sigma,\epsilon\}$$

$$+ \frac{\delta}{\delta x_\nu} \{\mu\sigma,\epsilon\} - \frac{\delta}{\delta x_\sigma} \{\mu\nu,\epsilon\} \tag{15}$$

This is the Riemann-Christoffel tensor. The contravariant index may be lowered, giving

$$B_{\mu\nu\sigma\rho} = g_{\rho\epsilon} B^{\epsilon}_{\mu\nu\sigma}$$

$B_{\mu\nu\sigma\rho}$ is anti-symmetrical in ν and σ and also in μ and ρ. It is symmetrical for the double interchange μ and ν, ρ and σ. It also has the cyclic property

$$B_{\mu\nu\sigma\rho} + B_{\mu\sigma\rho\nu} + B_{\mu\rho\nu\sigma} = 0$$

These properties reduce the number of its independent components from 256 to 20.

The Riemann-Christoffel tensor, as can be seen at once from (15) is built up wholly from the g's and their derivatives. Its importance for relativity theory lies in the fact that its vanishing gives the necessary and sufficient condition for flat space-time. That the condition is necessary is seen at once. For if space-time is flat it permits the use of Galilean co-ordinates. But for Galilean co-ordinates the g's are constant. Hence the 3-index symbols all vanish and from (15) the Riemann-Christoffel tensor vanishes also. But since it is a tensor it will continue to vanish in all other systems of co-ordinates. Hence for all systems of co-ordinates which can be used to describe flat

space-time the Riemann-Christoffel tensor vanishes. It can also be shown that the condition is sufficient.

Einstein's Law of Gravitation.

The Riemann-Christoffel tensor $B^{\epsilon}_{\mu\nu\sigma}$ may be contracted by putting $\epsilon = \sigma$, giving a covariant tensor of the second rank $G_{\mu\nu}$ formed wholly from the g's and their derivatives. From (15)

$$G_{\mu\nu} = \{\mu\sigma,\alpha\}\ \{\alpha\nu,\sigma\} - \{\mu\nu,\alpha\}\ \{\alpha\sigma,\sigma\} + \frac{\delta}{\delta x_\nu}\{\mu\sigma,\sigma\} - \frac{\delta}{\delta x_\sigma}\{\mu\nu,\sigma\} \qquad (16)$$

The law $G_{\mu\nu} = 0$ in empty space is chosen by Einstein as his law of gravitation. The reasons are as follows. It is assumed that, at great distances from matter, flat space-time can occur in nature. From (16) it is obvious that the law is fulfilled in this case. At the same time it does not, like the Riemann-Christoffel tensor vanishing, rule out everything but flat space-time. For $G_{\mu\nu}$ can continue to vanish even when the Riemann-Christoffel tensor does not. Thus it permits a limited variety of curved space-times, namely, those whose g's satisfy the equation $G_{\mu\nu} = 0$.

Further $G_{\mu\nu}$ contains no differential coefficients of the $g_{\mu\nu}$ higher than the second, and it contains those linearly. For these reasons one is justified in trying this law before proceeding to more complicated expressions.

The Three Experimental Results.

Strictly speaking, in order to make use of the equations $G_{\mu\nu} = 0$, we have to find a solution and then find what distribution of matter this solution represents. But for certain problems, *e.g.* the gravitational field of a particle, a solution can be obtained by generalising from the known form of ds for flat space-time. In this way the formula for ds in the neighbourhood of an isolated particle has been found to be

$$ds^2 = -\gamma^{-1}dr^2 - r^2 d\theta^2 - r^2 \sin^2\theta d\varphi^2 + \gamma dt^2 \qquad (17)$$

where $\gamma = 1 - \dfrac{2m}{r}$

The geodesic path of a particle in this kind of space-time is given by the equations

$$\frac{d^2u}{d\varphi^2} + u = \frac{m}{h^2} + 3mu^2 \qquad (18)$$

$$r^2 \frac{d\varphi}{ds} = h \quad \text{when} \quad u = \frac{1}{r} \qquad (19)$$

The equations to the path of a particle moving in the neighbourhood of an isolated particle according to Newton's law of gravitation are —

$$\frac{d^2u}{d\varphi^2} + u = \frac{m}{h^2} \tag{20}$$

$$r^2 \frac{d\varphi}{dt} = h \tag{21}$$

We see that the Einstein equations contain the extra term $3mu^2$. The ratio of $3mu^2$ to m/h^2 is $3\left(r\frac{d\varphi}{ds}\right)^2$. For ordinary velocities this is an extremely small quantity. The difference between ds in (19) and dt in (21) is also very small. ds is the *proper time*, that is, the time measured by an observer for whom the moving body is at rest. Einstein's solution, therefore, applied to the planetary orbits, will give a result very little different from Newton's. The extra term in (18) has the effect of giving a movement of perihelion. Mercury is the only planet for which this predicted movement can be tested, and in this case the observed movement of perihelion which was not accounted for on the older theory is, in magnitude and direction, precisely that predicted by these equations.

The path of a ray of light is a geodesic with $ds = 0$. From (19) if $ds = 0$, $h = \infty$, and therefore (18) becomes

$$\frac{d^2u}{d\varphi^2} + u = 3mu^2$$

An approximate solution of this equation is

$$u = \frac{\cos\varphi}{R} + \frac{m}{R^2}(\cos^2\varphi + 2\sin^2\varphi)$$

or, putting $x = r\cos\varphi$, $y = r\sin\varphi$, and multiplying throughout by Rr, —

$$x = R - \frac{m}{R}\frac{x^2 + 2y^2}{\sqrt{(x^2 + y^2)}}$$

This gives the measure by which the path of the light-ray departs from the straight line $x = R$. For a ray grazing the sun's limb this gives a deflection of 1.75″, a result confirmed by two eclipse expeditions.

Consider the time of vibration of a clock (*e.g.* an atom) at rest in this gravitational field. Since it is at rest we have from (17) as

$$dr = d\theta = d\varphi = 0.$$

$$ds^2 = \gamma dt^2$$

The time dt of vibration varies inversely as $\sqrt{\gamma}$. Clocks are said to be similar when they

beat out equal intervals. The times of vibration of an atom on the sun and of a similar atom on the earth ought therefore to be different since γ is different. An atom on the sun vibrates more slowly than a similar atom on earth. The lines of the solar spectrum, therefore, should show a systematic shift towards the red end of the spectrum. This result, also, has been confirmed by observation.

Mechanics and Geometry.

For a piece of matter let (u, v, w), assuming flat space-time and rectangular co-ordinates, be the velocity of the centre of the mass. Let the internal stresses be denoted by p_{xy}, etc., and let ρ be the density. It can be shown that the following expressions form the components of a contravariant tensor of the second rank, —

$$T^{\mu\nu} = \begin{matrix} p_{xx} + \rho u^2 & p_{yx} + \rho vu & p_{zx} + \rho wu & \rho u \\ p_{xy} + \rho uv & p_{yy} + \rho v^2 & p_{zy} + \rho wv & \rho v \\ p_{xz} + \rho uw & p_{yz} + \rho vw & p_{zz} + \rho w^2 & \rho w \\ \rho u & \rho v & \rho w & \rho \end{matrix}$$

The equations $\frac{\delta T^{\mu\nu}}{\delta x_\nu} = 0$ express the conservation of mass and momentum *for Galilean co-*

ordinates. The equations $\frac{\delta T^{\mu\nu}}{\delta x_\nu} = 0$ are not tensor equations, they are merely a compact way of writing those laws of nature called conservation of mass and momentum. In the enunciation of these laws Galilean co-ordinates are implied. But by taking the covariant derivative $(T^{\mu\nu})_\nu = 0$, we obtain a tensor equation, so that if this equation is true for one system of co-ordinates it is true for all systems that may be derived by mathematical transformations of that system. But the equation is true for Galilean co-ordinates, for in those co-ordinates the covariant derivative reduces to the ordinary derivative. Hence the equation is true for all co-ordinates derivable by mathematical transformations of Galilean co-ordinates. We shall assume, as usual, that the equation also holds when an irreducible gravitational field is present, that is, for other than flat space-time. By lowering one suffix we may write the equation in the form —

$$(T_\mu^\nu)_\nu = 0 \qquad (22)$$

These equations summarise the way in which the physical quantities mass, momentum, stress, etc., are observed to behave. But we can find purely

geometrical quantities which obey exactly the same equations and therefore behave in exactly the same way. These geometrical quantities are the components of a mixed tensor of the second rank, just as the physical quantities are, and the covariant derivative of this geometrical tensor vanishes identically. This tensor is —

$$G_\mu^\nu - \tfrac{1}{2}\, g_\mu^\nu G \tag{23}$$

where $G = g^{\alpha\beta}\, G_{\alpha\beta}$, an invariant.

It can be shown, by slightly laborious calculations, that $(G_\mu^\nu - \frac{1}{2} g_\mu^\nu G)_\nu = 0$. We can therefore make the identification —

$$G_\mu^\nu - \tfrac{1}{2} g_\mu^\nu G = -\, 8\pi T_\mu^\nu \tag{24}$$

where the factor $-\,8\pi$ is introduced to co-ordinate our units. Call T the contraction of T_μ^ν. Thus $T_\mu^\mu = T$. Contracting both sides of (24) we have, since $g_\mu^\mu = g_1^1 + g_2^2 + g_3^3 + g_4^4 = 4$.

$$G = 8\pi T \tag{25}$$

Substituting this value of G in (24) we have —

$$G_\mu^\nu = -\, 8\pi\, (T_\mu^\nu - \tfrac{1}{2} g_\mu^\nu T) \tag{26}$$

In a region where no matter exists all components of T_μ^ν vanish. T also, of course, is equal to zero.

Hence for empty space $G_\mu^\nu = 0$, which is equivalent to Einstein's equation $G_{\nu\mu} = 0$.

This result suggests a different way of deriving Einstein's law of gravitation and throws a new light on the meaning of that law. We start with the interval as our fundamental datum. From this we obtain the tensor $g_{\mu\nu}$. By pure mathematics we obtain from this tensor the tensor $G_{\mu\nu}$, and from this the mixed tensor $G_\mu^\nu - \frac{1}{2}g_\mu^\nu G$. We find the equations obeyed by the components of this tensor, and we find them to be the same as the equations summarising the observed behaviour of the physical quantities mass, momentum, stress, etc. Hence we say that the geometrical properties described by $G_\mu^\nu - \frac{1}{2}g_\mu^\nu G$ manifest to us as these physical properties according to the scheme (24). The equations (26) give an equivalent expression of this identification. The absence of matter is described by saying that $T_\mu^\nu = 0$. In this case $G_{\mu\nu} = 0$. Einstein's law of gravitation for empty space, therefore, is merely an expression of the fact that the space is empty. It imposes no restriction on the fundamental structure of space-time. If we have a region of space-time where

a system of co-ordinates can be used whose g's obey the equations $G_{\mu\nu} = 0$, then that region of space-time is empty. If a system of co-ordinates can be used whose g's do not make $G_{\mu\nu}$ zero, then that region is occupied by matter. Thus any kind of space-time can, theoretically, occur. The occurrence of some kinds means that matter is present, of others, that it is not. In actual fact, so far as our knowledge of nature goes, not all kinds do occur. Some kinds of space-time would correspond to distributions of matter of which we have no experience, such as matter with a density far surpassing the density of any substance known to us. The present theory throws no light on the non-occurrence of such distributions, any more than it throws light on the fact that matter exists in discrete particles.

FINITE SPACE

Einstein's Cylindrical Space.

The interval is given by —

$$ds^2 = -R^2 d\chi^2 - R^2 \sin^2 \chi (d\theta^2 + \sin^2 \theta d\varphi^2) + dt^2$$

The measured area of a sphere of radius $R\chi$ is $4\pi R^2 \sin^2 \chi$. There is a greatest sphere at the

distance $\frac{1}{2}\pi R$. As the distance increases the spheres contract, shrinking to a point at the distance πR. This is the greatest distance that can exist in this space. The total volume of space is —

$$\int_0^\pi 4\pi R^2 \sin^2 \chi \cdot R d\chi = 2\pi^2 R^3.$$

De Sitter's Spherical Space.

The interval is given by —

$$ds^2 = -R^2 d\chi^2 - R^2 \sin^2 \chi \, (d\theta^2 + \sin^2 \theta d\varphi^2) + R^2 \cos^2 \chi dt^2$$

The spatial part of this formula is the same as Einstein's, but the time behaves differently. For a clock at rest we have $ds = R \cos\chi dt$. Thus the time beaten out by any clock (*e.g.* the vibrations of an atom) varies as sec.χ. Hence the time intervals become longer as we recede from the origin and at the "horizon" $\chi = \frac{1}{2}\pi$, dt is infinite for any finite value of the interval. The time separating any two events at the horizon is infinite. The horizon is illusory in the sense that all points in space are on the same footing. For an observer at the horizon events at the origin would take an infinite time. But for light to travel from the

origin to the horizon and back again takes an infinite time. The horizon, therefore, cannot be observed.

The revised law of gravitation, giving a finite curvature at every point of space is $G_{\mu\nu} = \lambda g_{\mu\nu}$ where λ is a constant. In De Sitter's universe $\lambda = \frac{3}{R^2}$. Einstein's universe requires matter, and it is assumed (as a large-scale average) that this matter is distributed with uniform density. According to Einstein $\lambda = \frac{1}{R^2}$ and $8\pi\rho_0 = \frac{2}{R^2}$. Hence, from the formula for the volume of Einstein's space the total mass of matter in the universe is $M = \frac{1}{2}\pi R$.

PARALLEL DISPLACEMENT

Consider the change undergone by the components of a vector A_μ, when it is moved through the displacement dx_ν. In rectangular co-ordinates the change is given by $dA_\mu = \frac{\delta A_\mu}{dx_\nu} dx_\nu$. We may call this the physical change of the vector A_μ. It is the change we are concerned with in physics. We wish to express the rate of change in such a way

that, if it is zero in one system of co-ordinates, it is zero in all. This condition is not fulfilled by the above expression, since $\frac{\delta A_\mu}{\delta x_\nu}$ is not a tensor. Further, the tensor expressing the rate of change must reduce to $\frac{\delta A_\mu}{\delta x_\nu}$ for rectangular co-ordinates. These conditions lead us to choose the covariant derivative as corresponding, in all systems of co-ordinates, to what we have called the physical rate of change. It may also be called the absolute rate of change since it gives the total change minus the spurious change due to the co-ordinate system. A vector is said to undergo parallel displacement if it is displaced in such a way that no absolute change occurs. The condition is, therefore, from (8) —

$$\frac{\delta A_\mu}{\delta x_\nu} = \{\mu\nu,\alpha\}\, A_\alpha \quad \text{or} \quad dA_\mu = \{\mu\nu,\alpha\}\, dx_\nu A_\alpha \qquad (27)$$

The corresponding expression for a contravariant vector is, from (9) —

$$dA^\mu = -\{\epsilon\nu,\mu\} dx_\nu A^\epsilon$$

The notion of parallel displacement may be generalised for a more general space than

Riemann's by postulating that an infinitesimal vector A^μ, moved by parallel displacement through the displacement dx_ν, undergoes the change $dA^\mu = -\Gamma^\mu_{\nu\alpha}A^\alpha dx_\nu$, where $\Gamma^\mu_{\nu\alpha}$ represents 64 arbitrary coefficients. If we make the further condition that $\Gamma^\mu_{\nu\alpha} = \Gamma^\mu_{\alpha\nu}$ the number of independent coefficients reduces to 40. Like the Christoffel 3-index symbols, the $\Gamma^\mu_{\nu\alpha}$ are supposed to be functions of position and are not supposed to form a tensor.

What is assumed in this formula is that the change dA^μ of a vector is a linear function of the components of the vector. That is, we assume $dA^\mu = -d\gamma^\mu_\alpha A^\alpha$. We further assume that the coefficients $d\gamma^\mu_\alpha$ are linear functions of the differentials dx_ν, so that $d\gamma^\mu_\alpha = \Gamma^\mu_{\nu\alpha}dx_\nu$. The further condition $\Gamma^\mu_{\nu\alpha} = \Gamma^\mu_{\alpha\nu}$ means that if a displacement AB is equivalent to CD, then AC is equivalent to BD.

Movement by parallel displacement, or without absolute change, is the nearest we can get to a generalisation of the ordinary notion, which assumes rectangular co-ordinates, of absence of physical change. But it does not mean that the

vector at the end of its journey is identical with what it was at the beginning. A vector taken by parallel displacement round a small circuit does not in general coincide with its original position, although the absolute change vanishes. Similar considerations meet us when we wish to generalise the notion of a straight line for any kind of space, that is, when we wish to define a geodesic. We may say that a moving body describes a geodesic when its motion undergoes no absolute acceleration.

We shall take $\frac{dx_i}{ds}$ as the velocity of the body. We call this V^i. We may take s as a time-parameter: $x_i = x_i(s)$. In moving from $P = (s)$ to $P' = (s + ds)$ the vector V^i becomes

$$V^i + \frac{dV^i}{ds}\, ds$$

If the vector had moved without absolute change we should have had

$$dV^i = -\Gamma^i_{\alpha\beta} V^\alpha dx_\beta$$

The absolute change, therefore, is —

$$\frac{dV^i}{ds}\, ds + \Gamma^i_{\alpha\beta} V^\alpha dx_\beta$$

or, dividing by ds and substituting for V^i its value

$$\frac{d^2x_i}{ds^2} + \Gamma^i_{\alpha\beta}\frac{dx_\alpha}{ds}\frac{dx_\beta}{ds}$$

This is the absolute acceleration undergone by the moving body. If it is zero we get —

$$\frac{d^2x_i}{ds^2} + \Gamma^i_{\alpha\beta}\frac{dx_\alpha}{ds}\frac{dx_\beta}{ds} = 0$$

as the equations of a geodesic in this kind of space. In Riemannian space $\Gamma_{\alpha\beta}$ becomes the 3-index symbol $\{\alpha\beta, i\}$.

WEYL'S GEOMETRY

In Riemannian geometry the direction of a vector at B which has been moved by parallel displacement from A is not independent of the route of transfer from A to B. The length of the vector, however, remains unaffected. In the more general geometry of Weyl the length also changes. The length of a vector at B, moved from A by parallel displacement, depends on the route of transfer. Hence the standard of length at B which corresponds to a given standard of length at A is an ambiguous conception. We have

to lay down an arbitrary but definite gauge-system at every point of the continuum. At every point of space-time a standard of length is set up and intervals at that point are compared with it. The gauge-system, being necessarily arbitrary, may be altered.

We shall assume that the route of transfer need not be specified for infinitesimal displacements.

Let the initial length of a vector at P be l. Let it be moved, by parallel displacement, to a point P' infinitely near to P. The co-ordinates of P', relative to P, are dx_μ.

We assume that the change of length is proportional to the original length, that is, we assume $dl = \lambda l$, where λ is a function of position. We further assume that λ is a linear function of the co-ordinates, that is, we assume that $\lambda = \kappa_\mu dx_\mu$. Or we may write it $d(\log. l) = \kappa_\mu dx_\mu$. Suppose now we alter the gauge-system, the length of the standard at each point being decreased in the ratio φ, where φ is a function of position. The length of an interval at any point will be increased in the ratio φ. Thus, if accents refer to the new system $ds' = \varphi ds$.

The co-ordinates dx_μ, being merely identification numbers, are not altered by any change of gauge-system. Hence $dx'_\mu = dx_\mu$. Therefore $g'_{\mu\nu}dx'_\mu dx'_\nu = ds'^2 = \varphi^2 ds^2 = \varphi^2 g_{\mu\nu}dx_\mu dx_\nu = \varphi^2 g_{\mu\nu}dx'_\mu dx'_\nu$ Hence $g'_{\mu\nu} = \varphi^2 g_{\mu\nu}$.

Consider the expression for $d(\log.\ l) = \kappa_\mu dx_\mu$. We have $d(\log.\ l') = \kappa'_\mu dx'_\mu = \kappa'_\mu dx_\mu$. Also $l' = \varphi l$. Therefore

$$\kappa'_\mu dx_\mu = d\,(\log.\,l) + d\,(\log.\,\varphi) = \kappa_\mu dx_\mu + \frac{\delta\,(\log.\,\varphi)}{\delta x_\mu}\,dx_\mu$$

We may write $\log.\ \varphi = \chi$.

Then
$$\kappa'_\mu = \kappa_\mu + \frac{\delta\chi}{\delta x_\mu} \tag{28}$$

Put $F_{\mu\nu} = \dfrac{\delta\kappa_\mu}{\delta x_\nu} - \dfrac{\delta\kappa_\nu}{\delta x_\mu}$ Then $F'_{\mu\nu} = F_{\mu\nu}$

The expression $F_{\mu\nu}$ is unaltered by changing the gauge-system. It would appear that $F_{\mu\nu}$ refers to some intrinsic property of the world, since it is unaltered by change of gauge-system. Let us adopt ordinary rectangular co-ordinates so that $x_\mu = (x, y, z, t)$. Then

$$d(\log.\ l) = \kappa_1 dx + \kappa_2 dy + \kappa_3 dz + \kappa_4 dt.$$

If length, in the region considered, is to be

integrable, that is, independent of the route of transfer, $d\,(\log.\ l) = \frac{dl}{l}$ must be a perfect differential. The condition for this is $F_{\mu\nu} = 0$. If this holds good in one gauge-system it holds good in all. What is that physical condition of the world which varies according as the κ_μ do or do not satisfy the condition $F_{\mu\nu} = 0$? The field property not yet geometrised is the electro-magnetic field. This field is determined by the three components of a vector potential F, G, H, and a scalar potential Φ.

Suppose we make the identifications $F = \kappa_1$, $G = \kappa_2$, $H = \kappa_3$, $-\Phi = \kappa_4$. Then the equations $F_{\mu\nu} = 0$ become —

$$\frac{\delta H}{\delta y} - \frac{\delta G}{\delta z} = 0 \qquad \frac{\delta F}{\delta z} - \frac{\delta H}{\delta x} = 0 \qquad \frac{\delta G}{\delta x} - \frac{\delta F}{\delta y} = 0$$

$$-\frac{\delta \Phi}{\delta x} - \frac{\delta F}{\delta t} = 0 \qquad -\frac{\delta \Phi}{\delta y} - \frac{\delta G}{\delta t} = 0 \qquad -\frac{\delta \Phi}{\delta z} - \frac{\delta H}{\delta t} = 0$$

These are the conditions that, in the region considered, the electric and magnetic forces vanish.

We may say, therefore, that in regions where length is non-integrable an electro-magnetic field is present besides the gravitational field. In

regions where length is integrable, so that the geometry of the region reduces to ordinary Riemannian geometry, there is no electro-magnetic field. This generalisation of Weyl's is now regarded merely as an interesting and illuminating graphical representation of phenomena and not as a description of the actual geometry (the geometry ascertainable by the behaviour of measuring appliances) of the world. The reason is that the transport of material standards of measurement from one place to another is not a process analogous to the parallel translation of a vector. The Riemannian geometry of Einstein, on the other hand, describes the actual behaviour of material appliances.

We have seen that $g_{\mu\nu}$, on changing the gauge-system, becomes multiplied by φ^2. $F_{\mu\nu}$, on the other hand, is unaltered. This suggests that we may find tensors and invariants which are unaltered, not only by changes of the co-ordinate system but also by changes of the gauge-system. We should expect these quantities to be even more fundamental than ordinary tensors and invariants. They are called *in-tensors* and *in-invariants*. By

subjecting the 64 coefficients $\Gamma^{\mu}_{\nu\alpha}$ in the general formula for parallel displacement only to the condition $\Gamma^{\mu}_{\nu\alpha} = \Gamma^{\mu}_{\alpha\nu}$, we obtain 40 independent coefficients as against the 14 which characterise Weyl's geometry. This gives the more general geometry worked out by Eddington, and he has found quantities which are in-tensors and in-invariants for this more general geometry. Even from this very general relation-structure quantities can be deduced which behave in the same way as the quantities which characterise the physical world of perception. It can be shown, for instance, that the $\Gamma^{\mu}_{\nu\alpha}$ can undergo intrinsic changes (not merely co-ordinate transformations) without altering the perceptual world. One need only postulate, therefore, a very general type of relation-structure in order to deduce, mathematically, the world as it is.

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